THE SLING

REMARKS IN CONNECTION WITH LECTURES DELIVERED IN THE ROYAL INSTITUTION

LEIGHTON JORDAN

REVISED EDITION

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THE LAW OF GRAVITATION

SIR ISAAC NEWTON

POTENTIAL ENERGY

WILLIAM JOHN
MACQUORN RANKINE

PHOTOCOPY

THE SLING

LETTERS TO THE ROYAL INSTITUTION THE INSTITUTION OF CIVIL ENGINEERS AND THE ADMIRALTY MAY 1905 TO MAY 1912

BY

WM. LEIGHTON JORDAN

MEMBER OF THE ROYAL INSTITUTION; ASSOCIATE OF THE INSTITUTION OF CIVIL ENGINEERS; FELLOW OF THE ROYAL GEOGRAPHICAL SOCIETY, OF THE ROYAL SOCIETY OF ARTS, OF THE ROYAL STATISTICAL SOCIETY, AND OF THE ROYAL METEOROLOGICAL SOCIETY; PAST PRESIDENT OF THE ENGLISH LITERARY SOCIETY OF BUENOS AYRES

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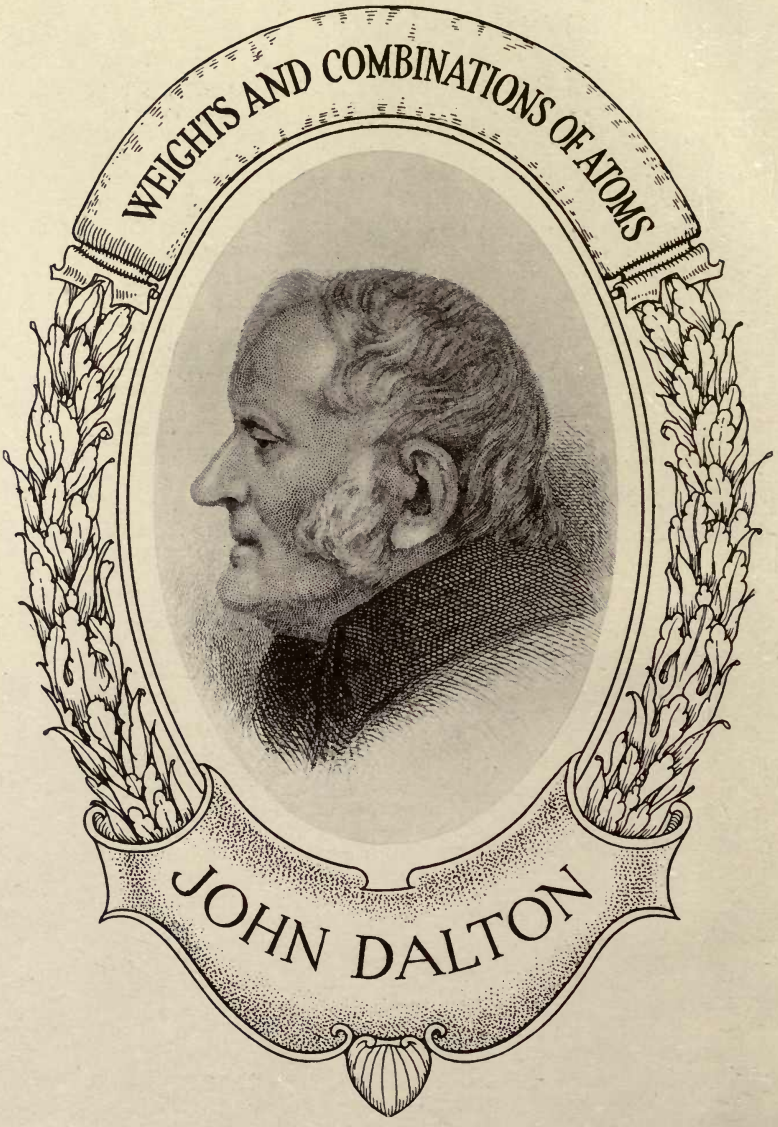
WEIGHTS AND COMBINATIONS OF ATOMS
JOHN DALTON

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PART I

LETTERS IN CONNECTION WITH LECTURES DELIVERED IN THE ROYAL INSTITUTION

MAY 1905 TO JUNE 1907

PREFACE TO PART I

In June last I printed for private circulation letters which I had addressed to the Royal Institution during the previous twelve months. After those letters had been printed I made the experiments which, in that of 23rd June, 1905, I had asked the Royal Institution to make, and the result is explained in my letters of 1st and 7th December, 1906, and in Appendix II. Some preceding letters which were made obsolete by those experiments are now omitted, and other letters added extending the review of the Institution Lectures to the following session.

The publication of the letters, not written for that purpose, seems to be justified by the importance of the errors attacked and of the new views suggested. The new discoveries in connection with the Sling, and their bearing on the subjects of 'Radiation Pressure' and the Pendulum would, even if they stood alone, suffice, I think, to justify the unusual form in which I have criticised the Royal Institution Lectures; but the reader will, I hope, easily see that they do not stand alone as points of vast importance in the history of natural philosophy.

Nec Deus intersit nisi dignus vindice nodus inciderit.

Royal Societies Club,
21*st October*, 1907.

LETTERS

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution

15th May, 1905.

Dear Sir,—Last Friday evening's lecture in this Institution constitutes an astounding occurrence. The able lecturer deserves our best thanks for his part in the matter, but, nevertheless, the fact remains that you have brought over from the United States one of our clever cousins to proclaim in our Lecture Hall, as a new discovery, a fact which had already been discovered here and established by explicit demonstration, whereas last night's treatment of the subject, instead of leading logically from cause to effect, was an avowedly unsuccessful attempt to apply a conjectural cause to phenomena rightly declared to exist, but which I am sure would have still remained unknown if an antecedent discovery of the true action of the forces on which they are dependent had not logically led to their discovery.

In astronomical questions, the force the lecturer dealt with as Pressure due to Radiation is, in fact, a combination of the transverse action of gravitation and reaction of astral gravitation which resists centripetal actions of gravitation; and without the antecedent discovery of that transverse action a knowledge of the effects could scarcely have been arrived at. The Professor's argument is brought forward only after the discovery of the fact that if any planet were lighter than it actually is, the sun's direct

force of gravitation would not be able to control its own transverse action in that planet's orbit, and the sun's transverse action would therefore lift the planet away from the orbit in which the heavier body retains its position.

The reason why the Professor was not able to show a clear logical connection between the facts he dealt with and Radiation Pressure is because no such radiation of heat through space of the character imagined does in fact exist. Interplanetary space is colder than ice. And it is only where the transverse action of the sun's gravitation acts on other bodies in space that heat is created by the clashing of their forces of gravitation. The sun's transverse action which creates the heat is itself the force which the lecturer treated as Pressure due to Radiation of heat. The heat is a consequence of the action of the forces by which the effects are caused, and not the cause of those effects. The idea of the sun parting with heat radiated across space to warm the earth is an exploded fancy. The sun, in fact, gets from the action of the earth's gravitation on it as much heat as the earth gets from the sun's action. Action and reaction are equal as regards that creation of heat in the two bodies.

The lecturer's argument to the effect that Newton's law of gravitation requires modification, shows him to be completely 'on the wrong track.' What that law has required is *extension* and not modification; and extension has been effected by the discovery of the action of astral gravitation and that of the transverse action of gravitation, with neither of which the lecturer professed to be acquainted.

24th May, 1905.

The subject dealt with in my letter of 15th instant is of such vast importance in the history of science that I wish more explicitly to define some of the points at issue.

I say that $\frac{m\,v}{r^2}$ (m = mass; v = radial velocity; and r = radius) is a universal formula for Newton's law of gravitation; and that the idea of the phenomena discussed by Professor Nichols, necessitating a modification of that law as he supposes, is erroneous.

I also say that $\sqrt[2]{\frac{m\,v^2}{r}}$ (v in this case being velocity of orbital motion) is a universal formula for orbital motion.

In the latter formula m is the central mass of any system together with that of the planet to be controlled by it. Taking m as the mass of any planet, $m\,v^2$ is the vis viva of orbital motion which has to be controlled by the centripetal force of gravitation. And as, in fact, the ratio which $m\,v^2$ bears to $\frac{m\,v}{r^2}$ increases as r increases, the sun's gravitation cannot control the vis viva of orbital motion without increments of planetary mass proportional to each increase of planetary distance from the sun.[1] The fact that the existing planets are controlled in their orbits by the reciprocal force of gravitation acting between them and the sun, necessitates therefore, for that purpose, in addition to the known masses of the planets and asteroids, the existence of material circulating round the sun equal in the aggregate to not much less than the mass of Saturn. And that material is wanted chiefly between the orbits of Mars and Jupiter inclusive.

Anything beyond the orbit of Neptune small enough to be a planet would either be annexed by Neptune as a satellite, or else be flung away from the system by the transverse action of the sun's gravitation unless given an unstable cometary orbit.

[1] This subject is further treated in Appendix I.

That excess of transverse action is what Professor Nichols treats as Radiation Pressure.

29th May, 1905.

If you have read with any attention the letters which I addressed to you on 15th and 24th instant, you cannot fail to have recognised that the discovery announced by our United States visitor on 12th instant is a detail of a vaster and more clearly defined series of discoveries already made here.

The opposition of the Royal Society to my views has thwarted direct recognition of them here, but a fragmentary echo from them brought back across the water was welcomed at the Friday evening lecture on 12th instant as a new discovery recently made in the United States.

This is not the first time that such an echo has gained attention in our scientific societies which were deaf to the original announcement made directly to them; and my present object is to point out another similar instance.

In a recent astronomical work based on lectures delivered in this Institution, Professor Turner tells us that about the year 1875 the method of dealing with astronomical questions was revolutionised by an American astronomer who discovered that mathematical work connected with the subject could be simplified by taking as a normal basis of the motion of any heavenly body a circular orbit round its primary, instead of a straight line tangent to that orbit, which latter had previously been taken as a normal basis for calculations.

The 'revolution,' described by Professor Turner, was in fact suggested by me in the year 1866, and it is only because the Royal Society, under the guidance of Sir

George Stokes, rejected a Paper on the tides which I offered to them in the year 1868 that the new method was neglected in this country and first given practical effect in the United States. In the year 1866 I pointed out that the *inertia* of the earth actually does keep it on the line of its orbit, resisting any motion from, as well as towards, the sun. The *momentum* of the earth's revolution tends to carry it off along a tangent to its orbit, but that momentum is incessantly resisted by the earth's inertia with which it has erroneously been confounded.

This is an important question in the history of science. And, in fact, my demonstration to the effect that inertia keeps the earth on the line of its orbit and does not tend to carry it along a straight line at a tangent to the orbit, preceded the abandonment of the straight line by practical astronomers as a basis for their calculations. The new method, which has revolutionised astronomy, justifies the argument I published in 1873 to the effect that—'the science of astronomy must be simplified and more easily extended by the use of this new power of reasoning from cause to effect than by the former process of demonstrating the necessary connection of concomitant effects proceeding from an unknown cause'—and history will not long continue to imagine the new method to have been arrived at except as a consequence of the correction of the erroneous theory of inertia by which astronomers had previously been guided.

The Royal Society have availed of and adopted the discovery made through discarding the so-called First Law of Motion without acknowledging their mistake in heaping disparagement on my initial action, though it seems absurd to suppose that the new method would ever have been discovered except in the manner I arrived at it

through first discovering that the action of astral gravitation makes any free action of that so-called First Law impossible throughout the universe.

5th June, 1905.

In continuation of the argument of my letter of 29th ultimo, I record that at the Friday evening lecture on 26th ultimo, Professor Bruhl, of Heidelberg, explained experiments which he considered proof of the dissolution of material atoms; and just two years ago, at another of our Friday evening lectures (19th June, 1903), Professor Curie, of Paris, had not only argued to the same effect, but also appeared to suggest that the phenomena which he described in connection with radium indicated a new material substance being in process of creation. Whatever the precise idea Professor Curie intended to convey may have been, the general tenor of his argument clearly showed that his authority could not be cited against the statement made by Professor Griffiths at our Friday evening lecture of 20th February, 1903 (just four months previously), to the effect that 'no physicist now relies with certainty on the conservation of matter.'

Professor Curie seemed to me to aim at a practical justification for that change in scientific opinion. At any rate, those lectures mark a clearly defined change of opinion since the time when Herbert Spencer fairly represented the scientific world in declaring that 'the indestructibility of matter is an indispensable axiom.'

Herbert Spencer goes on to say: 'The continuity of motion, like the indestructibility of matter, is clearly an axiom underlying the very possibility of a rational theory of evolution.'

Herbert Spencer on this point merely reflected what

was, so far as I know, the unanimous opinion of the scientific world when, in the year 1866, I initiated the doctrine of the 'evanescence' in substitution for that of the 'indestructibility' of matter. Herbert Spencer's view was that of the scientific world not only in his day, but had been so from time immemorial. When Newton declared that in the beginning God made primitive particles so very hard as never to wear or break in pieces, he did not pretend to be expressing merely his own idea or that of a particular school; but he was in fact adopting, or leaving uncorrected, what was then an accepted axiom in science which had long before been tersely summed up by Giordano Bruno in the words: 'There is nothing which by natural fitness is eternal but the substance which is matter.'

The crusade I inaugurated against that doctrine in the year 1866 has triumphed throughout the scientific world. The evanescence of what were supposed to be indivisible atoms, and not their eternity, is now a dominant basis of scientific argument, and has established a view of evolution vastly transcending anything dreamed of by the school of philosophy expounded by Herbert Spencer.

Notwithstanding that position of the question, the dictum of Sir George Gabriel Stokes and others to the effect that the views I had published were so wild as to place me utterly outside the pale of all reasonable scientific societies, seems to have so established itself in the minds of those even now in possession of our societies that the very men who are in fact proudly talking my dictation nevertheless fail to perceive that they are stultifying their position in the history of science by persisting in supposing me to be a 'paradoxer' properly outside the pale.

Dr. Williams, in his work 'The Story of Nineteenth Century Science,' alludes to Professors Stewart and Tait's inferences as to the destructibility of matter, and to Professor Dolbear's suggestions as to the creation of matter.

Dr. Williams there virtually gives to Stewart, Tait, and Dolbear credit for having originated the doctrine of evanescence which I announced, together with that of the retarding action of astral gravitation, in the year 1866.

My views as to matter being now in constant process of creation and destruction as a phase of force linking together pre-material and post-material forces, contributed quite as much as my denunciation of the idea of an 'initial push' given under conditions no longer existent, being a sufficient explanation of the existing planetary motion, towards setting in array against me all professional leaders of scientific opinion; and now, on those views being found worthy of consideration, they are falsely credited to Stewart, to Tait, and to Dolbear.

I urge our managers to make some effort to change a course which must ultimately bring discredit on all who have held positions of influence whilst it has prevailed.

10th June, 1905.

Sir William White was almost the last man worthily enrolled among the leading *experts* of the scientific world from whom I expected such an ungenerous attack on 'amateurs' as he made in last evening's lecture; and, for several reasons, a protest on behalf of 'amateurs' may properly be addressed to you by me.

First, the man most eulogised by Sir William White was Fulton, who was essentially an 'amateur' when he

framed the views which brought him into the position of the greatest expert of his age. When jeering at the mistakes made by 'amateurs,' Sir William White should have remembered that Fulton's achievements should (when distinguishing according to his method between experts and amateurs) be credited to the amateurs and not to the experts. I will not lengthen this argument either by adding other names or by specifying practical mistakes which may be debited to experts to make the balance even.

Secondly, one of the inventions illustrated last evening was ridiculed as showing the 'amateur's' ignorance of the true principles of wave motion. Does Sir William White pretend that any expert has yet mastered the principles of wave motion created as a ship ploughs its way through the sea? Or was the action of the tidal wave which accompanies every ship ever distinguished from the superimposed waves created by the ship's ploughing action before I (an 'amateur') addressed the Council of the Institution of Civil Engineers on the subject?

And, thirdly, in the year 1868 the Royal Society's experts rejected a Paper on the tides which I had based on a negation of the so-called First Law of Motion.

That law had never before that time been applied for practical guidance by shipbuilders. Only then Sir George Stokes and William Froude seized the idea that instead of discarding the law, as I proposed, it ought to be brought into more practical use.

I (an 'amateur') have given a clearly defined explanation of the manner in which erroneous theory, based on false principles, misled our shipbuilders into giving our yacht 'Shamrock III.' a shape which allowed her no chance against the American yacht 'Reliance' pitted

against her by builders less enthralled by a theoretical fetish; and until your proud experts have given some better explanation than I have done, they would do well to refrain from throwing stones at 'amateurs.'

23rd June, 1905.

In my letter of the 10th instant I alluded to the manner in which I have challenged the principles recently adopted by our professional shipbuilders; and my object to-day is to challenge, or, I should rather say, to express some doubt as to the correctness of generally accepted ideas regarding the action of a well-known instrument which, it appears to me, probably involves the same question as that at issue as regards resistance to a ship's way.

Though the SLING was a well-known instrument in war before the invention of gunpowder led to its displacement by the cannon for hurling projectiles, I find that text-books are generally shy as regards discussing the principles of projection it involves. Those which deal with it, however, expressly or tacitly, take it for granted that the vis viva with which the stone hurled from the sling strikes the object aimed at, is the kinetic energy created by the whirling motion and acting at a tangent to the whirl at the instant of the stone being released from the sling, supposing the motion to be circular at the instant of release.

I frankly state that I should like to have been able to make some more reliable practical experiments bearing on the subject than I have opportunity for, before suggesting that the above-mentioned idea may be erroneous; but, as that cannot be, I leave it to you and your brother experts to bring the question to a practical test when one of you condescends to think it possible that mistaken

ideas may perhaps prevail in text-books on the subject. I have corrected them on so many points that I do not grudge risking a mistake.[1]

Regretting that I am not able to make such experiments as to enable me to be absolute one way or another, I venture to say that I incline to think that the kinetic energy with which the stone moved whilst in the sling supplies only half of the vis viva with which it strikes its object: the other half having been merely potential energy whilst the stone was whirling in the sling, and becomes kinetic only on the instant of projection from the sling.

All the kinetic energy which existed in the sling at the instant of projection continues to act at right angles to that which was potential energy in the sling, and the tendency of the stone when released from the sling is to follow a diagonal between what were the tangent and the radius vector of its course at the instant of its release.

I suggest this diagonal, instead of tangental, course because, if found by experiment to be correct, the mistake which has existed regarding it explains why the fact of the sun's transverse action becoming uncontrollable by the direct action of gravitation in which it originates was not long ago recognised.

30th June, 1905.[2]

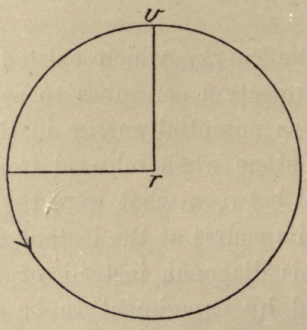
The main point on which I have addressed you is my contention that, in astronomical questions, $m\,v^2$ is the force which has to be controlled by $\frac{m\,v}{r^2}$ (v in the first formula

[1] See letter of 1st December, 1906, p. 45.

[2] Some parts of this letter, and some other letters, recorded and commented on, misinterpreted experiments with the Sling, and are omitted in this edition. Appendix II. shows what the mistake was. The accepted theory of forces acting in the Sling required correction, and the mistake was a stepping-stone to that correction.

being orbital velocity, and in the latter radial), and that the central mass, m, cannot control the actually existing transverse action, v^2, without planetary increments of mass increasing in the aggregate as the distance, so that not far beyond the orbit of Neptune any planet of sufficient mass or weight to prevent it from being hurled away from the system by the sun's transverse action would be destructive of the system.

Lord Rayleigh some time ago, in one of his lectures, showed us a whirling table contrived by Dr. Young, which



made it quite clear that the centrifugal force requisite for effective action on the bodies arranged on it is $\frac{v^2}{r}$. The bodies are placed on it between rails, which allow freedom only along the radius vector. The leverage of the physical radius, r v, not only supplies the motive force, but also prevents effective reaction against it, which has free play in interstellar space. That experimental mechanism of Dr. Young's, therefore, corroborates my argument to the effect that v^2 (and not $\frac{v^2}{r}$) represents the force which $\frac{v}{r^2}$ must control to prevent any body from being flung away from the central body of any system in space.

In Lord Rayleigh's experiment it appeared to me that the weight, when once started on its centrifugal course, traversed the radius vector without much effort to get away in either direction from it; and if so, then (as the radius vector continued to revolve) the natural tendency of the weight must be to take a diagonal course unless it does not reach the circumference of the table until the radius has described an angle of 90°.

The arguments I have brought to bear on the subject suffice, I think, to justify me in urging you to have the questions at issue definitely cleared up by experiment in one of the laboratories of the Royal Institution without waiting for some clever cousin to cross the Atlantic to tell us of a 'new' discovery in connection with the sling, as was done in connection with my substitution of v^2 for $\frac{v^2}{r}$ as the centrifugal force surrounding the sun.

3rd July, 1905.

In my published works I have so explicitly shown that the reaction of Astral Gravitation against motion does *not* tend to alter Newton's tangental action of momentum, that a diagonal course of a stone from a sling, instead of the tangental, would contravene my own writings as much as those of any text-book; and I have this morning made experiments under a different arrangement from those on which I have already commented, with the result that, unless I kept the plummet in the circle through about 45° *beyond* the point at which I endeavoured to release it, the course taken is along the tangent and not the diagonal.

My object has been to find how Newton's treatment of the subject can be reconciled with the fact of a transverse action of the sun's gravitation increasing its ratio

to the direct force as the distance increases, so that it ultimately becomes uncontrollable by the latter.

I am glad for the present, in the absence of direct evidence to the contrary, to be able to adhere to the tangental course of the stone from the sling; and I give, not as a mere suggestion, but absolutely, as the explanation of the apparent anomaly which on writing my letter of 23rd June I was bent on clearing up, that, under Newton's treatment of the moon's motion (which ignores any reaction against the motion), a velocity of 55 feet per second should suffice to counterbalance the earth's gravitation in the moon's orbit; but the effect of the reaction is that the normal velocity has to be multiplied by the radius, making it 3,334 feet per second, in order to counterbalance the earth's direct action.[1]

That same explanation holds good for the planets in relation to the sun, and shows how the velocity caused by the transverse action of the sun's gravitation gradually becomes too great to be controlled by the direct action in which the transverse action originates.

Our American cousin gave us in his lecture of 12th May, as a 'new' discovery made in the United States, a fragment of that excess of transverse action which had already been discovered here by a member of the Royal Institution.

21st August, 1905.[2]

The 'second point' alluded to in my letter of 12th instant, and left in abeyance, is as to the practicability of arranging an experiment to test the relative stresses along the radius A B and the tangent B T (Diagram, page 60), when a plummet at B is revolving in a circle B F.

[1] See footnote, page 52. [2] See footnote, page 15.

It seems to me that there must always be a sufficient excess of tangental force to keep incessantly pushing the revolving body from the diagonal B F to the circle B F. The motion along the circle appears to be the resultant of the tangental exceeding the radial stress as 146 is to 100; giving relative velocities as 121 is to 100.

To have a concrete, instead of merely an abstract, illustration of the question at issue, let B in the diagram on page 60 represent the moon revolving round the earth at A.[1] The moon traverses the quarter circle B F in 637,848 seconds, with a mean velocity of 3,334 feet per second; and consequently a mean velocity of fall of 2,123 feet per second from the tangent B T. But the force giving a tangental velocity along B T equivalent to the radial velocity B A would give a resultant motion along the diagonal B F. In order to lift the moon from the diagonal into the line of the circle the tangental force must be such as to give a velocity of 2,570 feet per second. The resultant motion of 3,334 feet per second is the square root of the sum of the two forces represented by $2{,}123^2$ and $2{,}570^2$.

The falling motion effected in the 637,848 seconds is not only the length of the radius, but also the incessant counteraction of the excess of the tangental over the radial force.

As those two forces are the radial and the transverse actions of the earth's gravitation, the resultant 3,334 feet per second is the effect of the full force of the earth's gravitation acting in the moon's orbit and giving that mean velocity of motion.

[1] The argument of this letter was an attempt to reconcile the Sling Forces with Newton's velocity of 3,334 feet per second, but the question has since been solved by the fact that the moon's velocity is 3,704 instead of 3,334 feet as supposed by Newton.

The velocity of 3,704 feet is confirmed by Rankine's Potential Energy, as shown in my letter of 15th May, 1911, in Part VI.

The foregoing is in practical agreement with Newton's demonstration of the force of the earth's gravitation in the moon's orbit; for the radius 1,261,000,000 feet shortened to 1,245,000,000 feet to reach the common centre of gravity in which the moon and earth would meet in falling together, and divided by ·00447 of a foot shown by Newton to be the radial fall per second in the moon's orbit, makes the distance $2{,}785 \times 10^8$ times greater than the fall in the first second; and the square root of that time gives 527,800 seconds as the time for falling under the unimpeded action of the centripetal force acting between the earth and moon to their common centre of gravity; but the lifting action of the excess of tangental force increases that time as 121 is to 100, making it 527,800 × 1·21 seconds = 638,600 seconds,[1] approximating to the time actually taken for the average tangental fall in each lunar quarter sufficiently for the purpose of the present argument.

8th January, 1906.

First, I am disappointed by the non-appearance of any official report of the very interesting lecture delivered in this Institution on the 12th May of last year, which was the subject of my letters of 15th May and some subsequent dates; and my attention has been drawn back to the subject by the beautifully arranged illustration of a comet's motion shown by Professor Turner on 28th ultimo, in one of the series of lectures with which he is at present delighting old and young in our lecture hall.

[1] In a letter addressed to the Institution of Civil Engineers on 13th November, 1905 (given in Appendix III. on page 63), I gave a more direct and simple reason for this difference of period. See also letter of 3rd May on pp. 34, 35. Newton ignored the excess of tangental over centripetal force, and balanced that error by orbital velocities corresponding with chords of arcs of 90 degrees instead of the arcs actually traversed as explained in the footnote on page 52.

I have in my published works from time to time pointed out that the interaction of the sun's radial and transverse forces of gravitation tends to pull each planet to pieces and distribute its particles in a ring round the sun. Each planet resists that action by the dominance of its own cohesive force of gravitation.

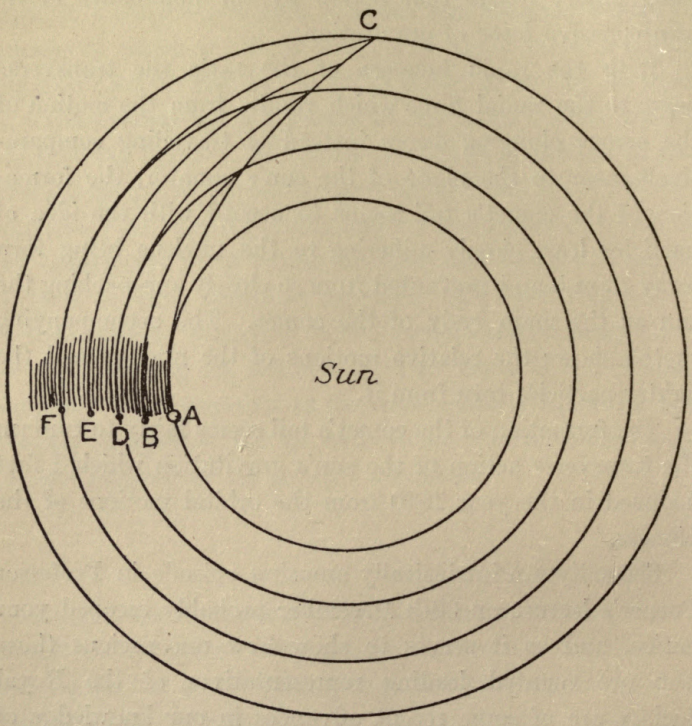
With the rapid increase of the ratio the transverse bears to the radial force which results from the motion of the comet plunging *across* instead of travelling comparatively near to the plane of the sun's equator, the formation of the comet's tail seems to accord with the idea of particles least closely adhering to the nucleus being torn away from it and prevented from so closely approaching the sun as the main body of the comet. The accompanying sketch shows the relative motions of the nucleus and the lighter particles torn from it.

The formation of the comet's tail seems fairly to confirm the transverse action of the sun's gravitation which I first deduced in the year 1870 from the orbital motions of the planets.

Secondly, an intrinsically amusing episode in Professor Turner's lecture on 30th December probably escaped your notice, and as it serves to show how unconscious those who are reputed leading representatives of the Royal Society are of some recent advances in our knowledge of physical phenomena, I think it permissible for me to call your attention to it; though I would not do so if it were not that it has also a serious aspect which serves to show how unfairly I am disparaged by those who are mistaken as regards the simple matters of fact on which they base the influential detraction against which I have for many years struggled.

In the course of one of his experiments the lecturer accidentally touched a PENDULUM which had been fixed

in readiness for a subsequent experiment; and when the time for the pendulum experiment arrived he expressed regret for the accident, fearing that it might have prevented the intended demonstration from being given.



C A Path of nucleus.
C B, C D, C E, C F } Paths of successive streams of dust which form the tail. (These streams are not in the same plane as C A.)

A wave of sadness passed through the juvenile listeners as the lecturer pointed out with regret that the pendulum did not swing quite straight; but that was quickly dispelled in a storm of delight when he announced that it was nevertheless moving in the right direction.

Well, in the first place, suppose the disturbance had been sufficient to make the pendulum begin by revolving in the wrong direction; the fact is it would, after a few swings, get rid of the bias accidentally given, and then automatically change to its right direction, and the desired demonstration would commence when the pendulum ceased to go the wrong way. Secondly, mathematically speaking, the curving from a straight line of vibration by which the juvenile andience were for a moment distressed is in fact an intrinsic part of the action of the forces by which the phenomenon of the revolution of the pendulum against the earth's motion is caused, though by steady starting with a long cord and short swing the deviation from the straight line is not immediately apparent; the action, however, steadily accumulates until the curve becomes apparent. Thirdly, for the purpose of the lecture it was unnecessary to keep the pendulum hanging in position for some hours previously and then to burn the string which held it in position. The pendulum may quite well be left hanging vertically, or swinging about, until wanted for the experiment; and if then drawn by the hand firmly against a starting-point marked on the heavy lecture table, as steady a start may be made as from the position of loose suspension from two cords which are always subject to more or less vibration.

The eminent representative of the Royal Society you enlisted to educate our juveniles is not up to date in the theory of the pendulum.

Thirdly, I now pass to a serious aspect of the performance.

In the year 1868 the Royal Society rejected a Paper in which I gave reasons why the main flow of the tide waves round the Great Southern Ocean should be eastwards—against the apparent motion of the sun and moon—and in

which I claimed that existing tidal records confirm those theoretical views.

There can be no doubt that Professors Stokes and Huxley, then secretaries to the Royal Society, were honestly and firmly convinced not only that my views were, in theory, wildly erroneous, but also that, in fact, the tide wave does not flow eastwards as required by my theoretical views.

A wave following the moon westwards round the Great Southern Ocean formed an important basis in all their accepted tidal theory. I have, however, shown that the Admiralty tide tables thoroughly confirm the eastward motion, and therefore call for a change of attitude on the part of the Royal Society towards the theoretical views which first indicated the existence of such a motion when it was generally taken for granted that those tide waves flow westwards as depicted on Dr. Whewell's charts.

I have elsewhere pointed out that those who conducted the oceanic explorations for the British Government after the publication of my work on the Ocean did not understand the question at issue. They, in fact, found vertical differences of temperature according with the theory of the circulating action of astral gravitation, but utterly at variance with preconceived ideas both of theory and of fact; and in the first published reports those differences were smoothed away on the supposition that they were merely consequences of rough treatment of the thermometers.

The treatment of the pendulum by Professor Turner in the Juvenile Lectures is analogous to the treatment of the thermometer by Sir Wyville Thomson and Dr. Carpenter in the Government Explorations. Professor Turner treated as due merely to rough treatment of the pendulum phenomena which really constitute the most exquisite part

of the effect of the earth's motion on it; just in the same manner as Sir Wyville Thomson and Dr. Carpenter treated as due to rough treatment of the thermometer phenomena which really constitute the most exquisite illustration of the conflicting action of terrestrial and astral gravitation in the ocean, which is caused by the earth's motion.

The attitude of Professor Stokes and other leading members of the scientific world towards my views is well shown by the following statement by Dr. Carpenter in the 'Contemporary Review' of September 1875.

Dr. Carpenter there says:

'Much ink has been wasted in the discussion of a question which the common sense of anyone who rightly apprehends the fundamental principles of physics should enable him to answer at once, viz., the influence of the earth's rotation upon the movement of the water which fills its ocean basins. This influence, supposing that water to be otherwise stationary, will be simply *nil*; for the water lying under each parallel will have the same rate of rotation from west to east as the solid earth in that parallel.'

The Secretary of the Royal Geographical Society thirty years ago informed me that the Council had decided that it was not desirable to encourage anyone engaged in commercial pursuits to meddle with scientific questions. That was also clearly the attitude of the Royal and other societies, whilst at the same time the above extract from the 'Contemporary Review' shows how I was handicapped in those commercial pursuits by the mistaken action of leading members of the scientific world.

The treatment of the pendulum in the Juvenile Lectures above alluded to, combined with that of comets' tails in the lecture of 12th May last year, entitles me, I think, to ask the managers of the Institution to make some effort

to change the attitude of the scientific world as regards the action of astral gravitation in terrestrial phenomena.

3rd March, 1906.

In his clearly expressed and extremely interesting treatment of the upward flow of sap in plants, Mr. Francis Darwin on Thursday afternoon declared the action to be still unexplained, as the fluid is, in fact, lifted in tall trees to a greater height than is compatible with the pumping action caused by the creation of a vacuum, and all efforts to trace a succession of pumping stages have failed.

I would not address you for the purpose of asking our managers to weigh any opinion of mine against that of Mr. Darwin on any point on which he has a definite opinion, but I cannot allow the statement that the rise of the sap, even in the tallest trees, stands unexplained to pass unchallenged in the lecture hall of the Royal Institution.

I say that the attempted explanations dealt with by Mr. Darwin have failed because the idea on which they are based, to the effect that the action is analogous to that of the vacuum created in a common pump, is erroneous. If there is any 'vacuum action' in the circulation of the sap, it must exist in infinitesimal detail, and only as a consequence of the direct action which is the true motive force.

Water is raised within a mountain by molecular gravitation preponderating over gravity, and is thus enabled to issue from the mountain-side high above the reservoir from which it is supplied. Sap not only rises vertically or flows horizontally by the same molecular action of gravitation, but also has, by some as yet unexplained mechanical action, the power of building channels by which it lifts itself, by the action of its molecular gravitation, away from the ground. And blood has the still

more highly developed power of completing a circulation in a single body by building channels for a return circulation which enable it to attain to some freedom of action by breaking away from visible bonds connecting it with the earth.

And now, sir, I must join issue with Mr. Darwin as regards a point on which he failed to do justice to a simple and thoroughly scientific opinion expressed by St. Paul. St. Paul argued that the perishing of the body is not proof against the continued existence of the soul, and gave as an analogous illustration the perishing of a seed resulting in the growth of a tree. A fair interpretation of his argument seems clearly to be a contention on his part to the effect that the perishing of the seed does *not* destroy its life, as is proved by the growth of the tree. It is only from that point of view that he is able to assert analogy as to the continued existence of life after the death of the body as well as after the death of the seed. Mr. Darwin's criticism of St. Paul seemed really to depend on a technical interpretation of the word *death* quite at variance with the idea which pervades St. Paul's argument. His argument is to the effect that the death of the *seed* is not the death of its *life*; and his view as to the breaking away of life from the body is merely a natural step following after the breaking away of the living body from the earth.

I beg, sir, that you will not lose sight of the fact that it is Mr. Darwin, and not I, who has brought St. Paul before the Royal Institution; and I take up the challenge because for forty years my scientific views have been frowned on by reputed leaders of Science, under the mistaken idea of agreement with St. Paul on the above point being unscientific.[1]

[1] A letter closely connected with the question of the upward flow of sap in plants is given in Appendix IV.

11th April, 1906.

It has become clearly inexpedient for me to refrain from addressing you on the subject of Professor Thomson's lectures of Friday, the 19th January, and the Saturdays of 3rd ultimo to 7th instant, as illustrating and confirming the general tenor of the letter which I addressed to you on 5th June of last year.

I then quoted Herbert Spencer's statement that:— 'The continuity of motion, like the indestructibility of matter, is clearly an axiom underlying the very possibility of a rational theory of evolution.'

That statement fairly represents the basis of scientific opinion as it existed when, in the year 1866, I began my public crusade against both of Herbert Spencer's so-called axioms.

What now has become of the doctrine—the 'axiom'— of the 'indestructibility of matter'?

The tenor of Professor Thomson's lectures made it appear that the doctrine of the *evanescence* of matter, rather than its *indestructibility*, is one of the foundation stones of his philosophy.

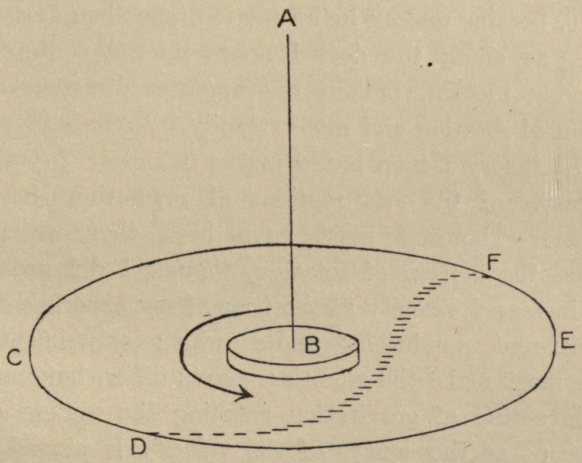
At any rate, even if Professor Thomson has not consciously advanced so far as to adopt the doctrine of evanescence unmarred by trivial limitations, he certainly has abandoned the so-called 'everlasting atoms' to destructive forces.

He did not appear to base any argument on the question as to whether those 'elementary atoms' are evanescent or indestructible, but, taking it for granted that they are evanescent, he attempted to explain possible processes under which they dissolve and the consequences of their decomposition.

That is all very well; but together with it, the resuscitation of Newton's corpuscular theory of light is a minor

episode in the part which the corpuscles supposed to combine to form Newton's 'indivisible' atoms are called on by their modern supporters to play.

Those minute particles are now called on, not only to dash through space from the sun to the earth at the speed of nearly 200,000 miles a second, but also to be, at the same time, flying in other directions for other purposes in the same field of action, and in such a manner as to



affect a heavy gyroscope so as to make it revolve faster in one direction than in the other.

I deny that there is any mystery as regards the force which causes the difference in the velocities of revolution pointed out by Professor Thomson, which, however, constitutes an interesting fact which has never before, so far as I know, been noticed.

Professor Thomson showed us that a suspended gyroscope, A B (in the Diagram), if tilted to the circle C D E F and given a motion of revolution in that circle, will revolve faster in one direction than in the other.

I some years ago made some rough experiments for the purpose of testing the theory of astral gravitation resisting all motion in the ratio of the square of its velocity. According to that theory the resistance is greatest when the revolution is in the same direction as the rotation, because the two motions combine to give an extreme excess of velocity on the outside edge of revolution over that at other points of the disc. And I found that in fact the disc, after revolving for a short time in the direction C D E F, became uneasy in its motion, and then crossed the circle, as along the dotted line F D, and continued its revolving motion, but in the opposite direction to the motion of rotation and more steadily.

The theory I then tested makes it appear to me that the faster of the two motions of revolution shown by Professor Thomson must have been that which was against the motion of rotation, though I did not notice that increased velocity at the time of my experiments.

Different conditions under which experiments are made must make the resulting effects differ, but until the simple action of gravitation resisting the motion of the gyroscope as the square of the velocity is proved to be unable to explain the phenomena under the conditions of the beautiful experiment placed before us by Professor Thomson, it is surely a mistake to appeal to mysterious action of mysterious 'corpuscles.'

27th April, 1906.

In my letter of 8th January, I commented on views expressed by Professor Turner in the course of his 'Juvenile Lectures.' I sincerely hope that my action in so doing is clearly recognised to be, *not* an attack on personal opinions of Professor Turner's, but on the whole school of science

which he very ably and worthily represented. The high position which you, sir, hold in the scientific world makes my criticisms quite as applicable to you personally as to Professor Turner; and still more strongly are those criticisms (made in a letter written in this Institution and addressed to you as Secretary) applicable to our official representatives of science, Lord Rayleigh and Sir James Dewar.

As, however, in the opening lectures of this year, Professor Turner was the protagonist of the old school of science in this Institution, I beg that, as a natural sequence to my comments on his lectures, our managers will not consider it altogether out of place for me to supplement my letter of 8th January with some comments on an article by Professor Turner which appeared in *The Times* on 24th instant, on 'The Times and Places of Earthquakes.'

1. Professor Turner tells us that the discovery of the existence of irregularities in the earth's rotation was made only twenty years ago.

As regards that statement, it is mere matter of fact that in April, 1879 (that is twenty-seven years ago), I addressed to Mr. William Spottiswoode, then President of the Royal Society, a Paper on the Moon's Motion, in which I declared such irregularities to be then 'a demonstrated matter of fact.' (That Paper forms now Chapter XIII. of 'The New Principles of Natural Philosophy': Longmans, 1885. See p. 250, line 17.) Let it be granted that Professor Turner is right to the extent that the great masters of the old school, who held complete control of the Royal Society, became aware of those irregularities only twenty years ago; then the fact is that the Royal Society tardily recognised the existence of a fact which the new principles had explained and announced to them seven years previously.

The theoretical action of forces which explained the facts before they were generally recognised is more entitled to consideration than explanations invented to suit them after the evidence of their existence had become indisputable.

2. Professor Turner tells us that there are two contributors to our earth's rotation, which sometimes act in unison and sometimes in opposition. I do not know which are the two contributors to which Professor Turner alludes; but I do know that the two *principal* contributors (besides the earth's own normal force of rotation) are the transverse action of the sun's gravitation which carries the earth round the sun, and the transverse action of a force of gravitation which carries the sun round the centre of our stellar system.

As regards the transverse action of the sun's gravitation which changes its direction in relation to the earth's rotation in semi-annual cycles, and the velocity of motion in annual cycles, the outer crust of the earth has doubtless settled into a condition of stability. But that stability is disturbed by the interaction of the transverse force of gravitation which revolves our stellar system in a period of 26,000 years, giving a maximum and a minimum velocity in that period just as the sun's transverse action gives a maximum and a minimum velocity each year. The gradual change of the velocity of stellar revolution in the cycle of 26,000 years is apparently the cause of the present disturbances.

Besides the change of velocity there is also a change in the direction of that revolution in relation to the earth's axial rotation and orbital revolution, which occurs twice in each of those cycles.

3. Professor Turner tells us Professor Jeans has suggested that 'the earth is by no means a sphere or

a spheroid, as we have been accustomed to think, but is of a pear-shape'; and that 'the pear is being crushed into a sphere by its own attraction; and the result is a series of earthquakes.'

It is now forty years since I published the work in which I pointed out that the motion of the earth through space *tends to give it a pear-shape*, and that the actual deviation of its shape from a true spheroid indicates that such motion is southwards—the Antarctic Continent rising from the Great Southern Ocean being the 'stalk' end, and the Arctic Ocean surrounded by the Northern Continents being the 'bluff' end of the pear. That should be the *longest* diameter in order to constitute a true 'pear-shape'; but, in fact, the oblate spheroid caused by the earth's rotation is not sufficiently disfigured to keep that line from being the *shortest* diameter of the 'pear'; and I applied the term cardioid to the shape which actually results from the interaction of forces.

Since I first pointed out that cardioid shape, Nansen has responded to my call for water in the Arctic regions by discovering a vast extent of deep sea there; and my call for land in the Antarctic regions has been responded to by Scott's discovery of King Edward VII. Land and a great extent of mountainous land in still more southern latitudes; whilst Drygalski has responded by discovering Gauss mountain, where he expected to find an oceanic pathway from the South Indian Ocean to the South Pole.

Professors Jeans and Sollas place the ends of their 'pear' in the South Pacific Ocean and in North Africa; but Professor Turner's question as to which is the 'stalk' end makes it appear that the pear must have become sadly squashed out of 'pear-shape.' It seems clear to me that they are in fact dealing with the ruins of a former cardioid, which existed when the direction of the earth's

motion through space differed from what it now is in relation to the earth's surface.

If we suppose the last change of direction to have occurred at the time of the Noachian deluge, some thousands of years have still to elapse before another change of direction occurs; and even that mighty change of conditions in many parts of the world perhaps left China and some other regions comparatively little affected, in which case interesting records probably lie forgotten in the Mukden Library.

Be the merits of the latter point, however, what they may, Professor Turner's very interesting article brings into strong relief important points in scientific research where he and other masters of the old school have only recently begun to tread in footsteps which had long before been made by the new theory of Vis Inertiæ.

3rd May, 1906.

I was greatly interested in the remarks you made on Tuesday at our annual general meeting on your position as honorary secretary. It appeared to me that they were perhaps inspired by the letters I have recently addressed to you; be that, however, as it may, they virtually formed a renunciation of responsibility on your part for the treatment of those letters, and, I think, an expression of opinion to the effect that not only the managers but also Sir James Dewar (as practically their representative) are responsible for conducting the affairs of the Institution. I am glad to think your remarks a step towards obtaining attention for the scientific questions I have brought before you; but, at the same time, I cannot see that you are entitled to repudiate responsibility as regards their treatment by the Institution. The Year-Book of the Institution, lying here on the table,

declares you to be the 'Secretary.' My appeal is, therefore, naturally made through you as secretary; and, independently of that office, your position in the scientific world is such as to enable you to command attention, so that you cannot free yourself from personal responsibility.

With what I have placed before the Institution in works now in the library, and in these letters, we have probably elements sufficient, if dealt with by men of the mathematical knowledge and ability of Lord Kelvin or Lord Rayleigh, and perhaps some others among our managers and professors, to trace the orbit of our stellar system.

Two among many other important questions which will almost certainly be determined by the solution of that orbit (as they appear to be naturally dependent on it) are :—

1. The reason for what is otherwise an anomaly in the period in which the moon describes its orbit of revolution round the earth.

According to the data made use of by Newton in solving the law of gravitation, the moon falls ·00447 of a foot per second from the tangent to its orbit, and therefore falls in 6·1 days the length of the radius of the orbit it describes round the common centre of gravity of the earth and moon; thus describing a quadrant in that time, and a complete circle in 24·4 days, whereas the period of its sidereal revolution is 27·32 days, and that of its synodical revolution 29·53.

I know of nothing that can give a clear and simple reason for the first of the above three periods, unless the moon in fact returns each 24·4 days to similar positions in relation to the centre of gravity round which our stellar system revolves. It then takes about three days to catch up with the motion of the visible stars round that centre of gravity, and about $2\frac{1}{5}$ days more to catch up with the motion of the earth round the sun.

2. The eccentricity of that stellar orbit serves to explain the existence of the glacial periods which have been indicated by geological researches. Existing evidence is to the effect that the solar system is now traversing the part of its orbit which corresponds to the vernal equinox of the Southern Hemisphere in the earth's orbit round the sun.

14th May, 1906.

I know it is dangerous to comment on an elaborate scientific argument before it has been placed on paper; but the main points of the important lecture delivered on Friday evening by Professor Poynting can scarcely be open, I think, to any misunderstanding.

In the first place, the argument clearly aimed at rebutting existing attempts to resuscitate Newton's corpuscular theory of light; and I venture to think it will prevent us from hearing any more talk of material particles dashing through space at the rate of nearly 200,000 miles per second.

If those tiny corpuscles cannot do the comparatively simple work which Newton demanded of them, still less can they reasonably be supposed competent to affect a heavy gyroscope in the manner required by their modern champion. Professor Poynting has indeed thoroughly justified the comments I submitted to you on the very interesting and able exposition which Professor Thomson gave of the corpuscular theory in his recent lectures.

Secondly, Professor Poynting explained to us how he and others had succeeded in measuring the amount of force exerted by the pressure of light. I know no reason why any student of science should be otherwise than predisposed to accept and welcome such an announcement. But, when Professor Poynting goes on to say

that the demonstration of a force of pressure exerted by sunlight constitutes a modification of Newton's law of gravitation, I join issue and renew the protest made in my letter of 15th May, 1905, when Professor Nichols made a similar claim. Newton's law remains perfectly true, no matter what other forces may be acting on any body at the same time.

When a book is lying on a table Newton's law is perfectly true as regards the book, even though the strength of the table prevents it from causing the book to move.

If Professor Poynting (also Professor Nichols) agreed with me that a transverse action of the sun's gravitation is the cause of its light, it would then be more plausible for him to urge that its light modifies Newton's law for the action of its gravitation. But the fact is that Newton's law must be treated independently of the interaction of any other laws, not excepting even that of the transverse action of the same force of gravitation. What requires modification is our estimate of the relative masses of the sun and the earth and that of any pebble lying on the earth, as pointed out in a letter which I addressed to the Royal Society on the 21st August, 1901. To modify Newton's law instead of those estimates would not only lead to endless complications, but is also an intrinsically erroneous method of dealing with the question. Gravitation is as the mass and inversely as the square of the distance: and any transverse action of a part of the force does not modify that law, but forms a supplementary law.

Thirdly, as regards the book lying on the table: it is quite allowable for any one who chooses to do so to deal mathematically with the pressure existing between the book and the table instead of with the action of gravitation, which I (and I think most people) say is the cause

of that pressure. But the law of gravitation, nevertheless, remains quite true as regards that book.

The transverse action of the sun's gravitation acting on the earth is just as truly a *pull* along the tangent as is the direct action a pull along the radius vector; and pressure between the sun and the earth, whether manifested as heat, as light, or as electrical action, is a consequence of the reciprocal action of their gravitation just as truly as the pressure between the book and the table is a consequence of the reciprocal action of gravitation between the book and the earth.

So-called ether pressure in space is, in fact, merely the opposite aspect of (or a consequence of) the tension of astral gravitation which pervades the universe.

23rd May, 1906.

As the subjects of the lectures to be delivered by Professor Sollas on the afternoons of the next three Thursdays are closely connected with that of a very interesting paper contributed by him to the Geological Society, and published in the 59th volume of their Quarterly Journal (May 1903), and as I pioneered, forty years ago, some of the most important points dealt with in that paper, I submit some comments in sequence with the course of letters originated by the misappropriation of my work in the lecture hall of this Institution, as explained in my letters of 15th May, 1905, and some subsequent dates.

In the discussion of the above-mentioned Paper by the Geological Society, the President (Professor Lapworth) asked why those who inferred an original pear-shaped figure for the earth did not notice that the present spheroid of rotation is somewhat pear-shaped—the Arctic regions being the blunt end, and the Antarctic the stalk end of the pear.

The fact, sir, is that the present treatment of the subject in the Royal Society and in the Geological Society is based on suggestions which were scouted when I first published them forty years ago.

The tendency of the oblate spheroid to deviate towards the so-called pear-shape is shown in a diagram impressed on the cover of a book which I published in the year 1866; again on another book published in 1867; on a third published in 1868; on a fourth published in 1873; and it is repeated in the frontispiece of the Essays on the Action of Astral Gravitation which I published in the year 1900, besides being the subject of various diagrams in previous works. The importance I attached to the point is shown by the prominence I gave to it as above stated, and my opinion of its importance has become justified by the so-called pear-shape having become the subject of numerous Papers in the scientific societies of this and other countries.

I did not use the term 'pear-shape' because, in its original position, it would be a 'pear' broader than its length; and because it is not merely 'blunt' at one end, but has a subsidence at that end; and I therefore used the term cardioid. The conformation of the cardioid becomes elongated so as to be more in accordance with pear-shape by a change of its position in relation to the earth's rotation.

Professor Sollas points out that a line from South America to the East Indies gives 'roughly a pear-shaped form.' And, in fact, in 1886 I gave reasons for supposing that Brazil had formerly been an Antarctic Continent. In such case the then existing Arctic Ocean would have been more or less south-east of the Japan Islands. The nations of some parts of Asia would have had the same climate as under the now existing

conditions ; but the Japanese would have been the ' Eskimos ' of that era.

I also gave reason for supposing that traces exist of changes antecedent to the above to account for the absence of more land in the South Pacific Ocean.

Professor Sollas supposes a pear-shape to exist with the stalk end in Northern Africa and the blunt end in the South Pacific, and that the recent succession of earthquakes is due to the action of gravity tending to crush that pear-shape towards a spherical form. I have a perfectly free and open mind for anything Professor Sollas may urge in support of that idea. The evidence on which my work has been based, however, indicates that at least about 19,000 years must have elapsed since a tendency to create a pear-shape in that position existed ; and the now existing stresses between the forces which create the existing oblate spheroid and those which give it the existing deviation towards the cardioid, or ' pear-shape,' alluded to by Professor Lapworth, give a readier explanation of the slight instability of the earth's crust now becoming a very general subject of consideration.

26th May, 1906.

Verba volant : warns me of the dangers which beset the path I am about to traverse in commenting on the important and intensely interesting lectures Professor Sir James Dewar has delivered on the last two Saturday afternoons on ' The Old and the New Chemistry.'

In commenting on Professor Poynting's recent lecture, I suggested that it would put a stop to the attempt to resuscitate Newton's corpuscular theory of light, considering his argument a death-blow to that attempt. Sir James Dewar, in refraining from standing by as a silent

spectator of the tragedy, has (as it seems to me) shown a very proper sense of the responsibilities of the high position he has attained in the scientific world.

There did not appear to me to be too much ceremony in Professor Poynting's cuts and thrusts; but, even if he was rather heartless in the affair, Professor Dewar, acting as chief mourner, has amply atoned for it by the tones of regret with which in solemn and slowly-measured language he limited material particles to a velocity which would not enable them to move at more than two-thirds of the pace requisite to enable them to be the Newtonian corpuscles of light. The corpuscles are now, indeed, doubly dead as regards their pretension to constitute light; but what has Professor Dewar to say of their pretension to be the controllers of the gyroscopic phenomena in the beautiful experiment which Professor Thomson placed before us in his recent lecture, alluded to in my letter of 11th April? That appears to me to be a more startling pretension than their claim to be light. Just think, sir, of those tiny, invisible particles being in a chronic state of dashing about the lecture hall with such fierce velocity as to create the phenomena of the gyroscope shown to us by Professor Thomson! I beg you, Sir William Crookes, together with your managers and their professors, to open your eyes and recognise that there is no basis nearer than the stars for the tension which controls the movements of the gyroscope.

There is much of interest in Sir James Dewar's lectures with which it would be imprudent to grapple before seeing it on paper. In general terms I may, however, I think, say that he virtually agreed with the views of Professors Bruhl, Griffiths, and Curie, expressed in our lecture hall and alluded to in my letter of 5th June last; and, passing to a more clearly-defined position than held

by them, his exposition of the electrical basis of matter, combined with his limitation of corpuscular velocity of motion, is virtually a declaration of transmutations between material substance and immaterial force dealt with in my essay on ' Sir Isaac Newton and Modern Chemistry,' and constitutes an adoption of the doctrine of the evanescence of matter which I stood alone in upholding forty years ago.

14th September, 1906.

Verba volant: which I quoted in the last letter which I addressed to you, conveys a warning which could not easily be more splendidly justified than it has been by the abstract of Professor Nichols' Paper on Pressure due to Radiation, which you have just distributed among the members of our Institution and published.

The attack made on Newton's Law of Gravitation in our lecture hall on 12th May, 1905, remains: *vox et præterea nihil.* The able young Professor you put forward to lead the rebellion against that Law, on that which was evidently intended to be an epoch-making evening, has (if he is responsible for the abstract) thrown up the sponge and left the rebellious crowd who supported him on our front benches without an apparent leader. Will they all be content to subside like the foam of a wave falling back repulsed by an adamantine rock? Or will you, sir, organise another attack?

Newton demonstrated the Law of Gravitation in the only manner in which it was possible for it to be arrived at: namely—by its direct action, and not through the varied forms of pressure which that action creates.

I again urge you, Sir William Crookes, together with your managers and their Professors, to open your eyes and recognise that Radiation Pressure in all its forms is merely

the opposite aspect of the action of astral gravitation which pervades the universe.

21st September, 1906.

In a letter dated 14th instant I commented on the omission from the report in our Proceedings of what, when the lecture was delivered by Professor Nichols on 14th May, 1905, appeared to be intended by the lecturer to be the climax of his argument; which was certainly then declared to require a 'modification' of Newton's Law of Gravitation.

The report of the lecture leaves Newton's law untouched; and I now venture to make some comments on the *débris* which has been printed in our Proceedings.

The abstract tells us that the first speculations upon a possible pressure due to radiation were suggested by the behaviour of comets' tails; but it refrains from printing the verbal description of—'the enormous velocity with which the matter of the tails is carried backwards, in opposition to the motion which it had as part of the nucleus.'

In the absence of the words used by Professor Nichols I give the above from Sir John Herschel; and I notice that, applied to the comet of 1843, the argument would require a velocity of nearly 200,000,000 miles in nearly no time.

Sir John Herschel expresses a doubt as to whether 'it is really *matter*, in the ordinary acceptation of the term, which is projected from the heads of comets with such extravagant velocity'—'in defiance of the law of gravitation, nay, even of the received laws of motion.'

The fact is that (subject, perhaps, to a trivial correction) the so-called enormous and extravagant velocity is no velocity at all: the velocities are round the sun, *not*

from the sun. As the comet plunges across the plane of the sun's rotation the transverse action of the sun's gravitation takes complete control of the dust torn away from the comet; which latter continues its course under the *combined action of the two* forces (its own and the sun's); and as the comet returns to recross the sun's equator it gathers up its tail as the particles continuously meet the comet as the latter recedes again from the sun.

The action does *not* defy, but constitutes a verification of the laws of gravitation; and as for the so-called First Law of Motion, the comets laugh it to scorn. Herschel is right as to the comets setting it at defiance.

In the year 1866 (forty years ago) I gave that law an initial impulse which has set it going, with a velocity which is rapidly accelerating, to the limbo of exploded fancies; and if you, Sir William Crookes, and our managers, persist in holding on to it, it will take all your reputations with it.

24th September, 1906.

The abstract of Professor Nichols' lecture declares him to have given a satisfactory explanation of 'the hitherto mysterious behaviour of comets' tails.'

That alone, quite independently of the question as to whether that explanation did, or did not, necessitate a 'modification of Newton's Law of Gravitation,' was enough, if the explanation were sound, to make the lecture an epoch-making event in our Institution.

The boasted explanation has not, however, dared to show itself in print: *Scriptum manet*; and you, sir, must, I think, now be conscious of the sorry figure it would have cut if you had allowed me to get hold of it in that form.

Kepler did not allow himself to be hampered by the so-called First Law of Motion; and he seems to me to have

been nearer to a true explanation of the phenomena than any ' orthodox ' scientist has since been.

In face of the above I think it not out of place to remind you that on 13th of March, 1898, I appealed to the managers of our Institution not to allow the Nineteenth Century to close (with the 31st December of the following year) [1] without Astral Gravitation having been fairly discussed in any of the professional seats endowed for the advancement of scientific knowledge. In making that appeal I believe I was considering the general interests of science as much as the immediate question of fair-play for the extended laws of gravitation in their struggle against the so-called First Law of Motion.

1st December, 1906.

In my letter of 30th June, 1905, I appealed to you for enlightenment regarding two points in connection with the Sling; and I desire now to advise you that I have just arranged mechanical experiments which appear to set both questions at rest.

First: the plummet released from the sling *does* follow a tangent to the curve in which it has been revolved; thus leaving generally accepted ideas quite correct on that point.

Secondly: the ratio which tangental bears to radial force is *not* one of equality, but is as 146 is to 100.

This ratio of forces holds good not only as regards a railway train rounding a curve, but also as regards planetary motions in their orbits; and the suggestions I have made regarding it do not, I think, in any way overestimate its vast importance.

[1] In a small volume on 'Astronomical and Historical Chronology' I have shown it to be a simple historical fact that, when the Christian era was invented, what we now call 1 B.C. was the first year of the era and 1 A.D. the second year of the era; so that the first year of the era *closed* as 1 A.D. in our chronology *commenced*.

Dr. Young's mechanical contrivance, alluded to in my above-mentioned letter, would tend to carry the bodies flung from it by centrifugal force (beyond the limits of his Whirling Table) the course which I ventured to suggest in connection with the sling : namely, not along the tangent, but along a line diverging about 40° from the tangent. I made some experiments which gave a divergence of about 30°, the difference being apparently due to friction ; and I this afternoon arranged some mechanism which I hoped would make the friction practically almost a negligible quantity (as I believe it is in Dr. Young's mechanism), but my mechanism was not strong enough for the test, and gave way ; so the angle has not been perfectly demonstrated by experiment, but the evidence is sufficient to justify a repetition of my request for the point to be demonstrated in one of our laboratories.

7th December, 1906.

As, in my letter of the 1st instant, I ventured to ask you kindly to have made in one of our laboratories the experiment which I had not succeeded in bringing to a clear issue on that afternoon, I think it right to advise you that I have since succeeded in effecting it, and the rough lines showed the angle of divergence from the tangent to be 47 to 48°.

That angle I take to result from the fact that the motion of the body along the revolving radius of the table is one of equable acceleration ; so that, if started from a position placing its centre of gravity in the axis of rotation, it would on reaching the periphery of the Whirling Table, have radial and tangental momentum in the ratio of 200 to 121.

That, by theory, should give an angle of about 59° ; and the difference may be accounted for partly by friction, but

chiefly by the practical difficulty of starting the body with its centre of gravity in the axis of the Whirling Table's rotation.

It seems to me that in a well-arranged experiment there would be no friction, but merely hammering action on the body as it traverses the revolving radius.

If you make the experiment I shall be much obliged if you would kindly let me know the result.

8th June, 1907.

Sir James Dewar's lecture last evening was splendid; it eclipsed all that I had before heard from him.

Of course he knew that the experiments he showed us were of first-class importance; but if he had shown the slightest consciousness of the fact that before he was half through the lecture the Vibration Theory of Light was dead, and that in the exquisitely managed experiment with the vapour of mercury he was merely slaying the slain, I should not be writing to you on the subject.

It was also clear that neither Lord Kelvin nor you (or many other mighty listeners) recognised what a great epoch-making event in the history of Science had occurred.

This could not have been so if it were not that the slaughter was indirect. A *new* and *the true* theory shone through the experiments; and the Vibration Theory peacefully vanished in its presence.

Of course we all know that the attempt made last Session to revive the Corpuscular Theory merely resulted in showing it to be doubly dead; but who then dreamed that the Vibration Theory was so soon to follow its rival to the limbo of exploded fancies!

10th June, 1907.

As I walked along Albemarle and St. James's Streets just after Sir William White's lecture had closed the Session of the Royal Institution on Saturday, a cloud of history rose around me. I realised what a triumph Sir James Dewar's lecture of the previous evening was for Sir Isaac Newton, whose sturdy thought had firmly kept him from ever accepting the Vibration Theory of Light, even when he became shaky as regards his Corpuscular Theory. I reflected also that it was the restless amateur Rumford, one of our enterprising American colonists, who founded and organised the Institution which had made such a lecture as that of Friday evening possible. The doors of that Institution, founded by the amateur Rumford for the free consideration of all questions connected with Natural Philosophy, are now slammed in the face of any criticism of the so-called First Law of Motion, which was in fact relegated to the limbo of exploded fancies more than forty years ago. I never had any special spite against that law ; it was a clever and simple invention, which served the purpose of those who invented it, and is also the basis of a vast amount of the most abstruse mathematical demonstrations published by leading scientific societies. I had no special wish to disturb its proud position ; I chanced, as an independent student of Natural Philosophy, not cramming to pass an Examination, but studying to arrive at the truth for my own satisfaction, and unhampered by the necessity of keeping in line with all that had come to be looked on as established principles in science :—in that freedom I recognised the existence of the innumerable forces of tension caused by the connecting links of gravitation reaching from star to star, and forming by its multitude of lines of force everywhere a plenum of interacting forces which makes the supposed action of the

so-called First Law impossible throughout the material universe.

I will now place before you only one field of action in which that discovery of the action of astral gravitation has had practical effect.

Sir George Gabriel Stokes, William Froude, and some others, not only scouted my attack on the 'First Law,' but woke up to the fact that it had never been given its due consideration as a practical law in terrestrial physics. They made up their minds that nobody had ever before understood the theoretical principles under which a ship should be constructed; and Stokes directed his unrivalled mathematical skill to show what modifications of accepted ideas of shipbuilding were required by that so-called fundamental principle of physics.

For a long time I really did not know what was going on; but after I became aware that the so-called First Law was being given practical application in the construction of our battleships and cruisers, I quickly found that erroneous principles had resulted in giving our ships too deep a displacement. I opened fire on our Naval Constructors through the Council of the Institution of Civil Engineers, partly because Sir William White, late Chief Constructor of the Navy, was a member of the Council, and partly because, as an Associate, I considered myself entitled to a hearing in that Institution.

About two years afterwards the keel was laid for what became known as the 'Secret Ship,' because, though avowedly to have great novelty in construction, the character of the change was kept dark. In about another year she was launched as the 'Dreadnought,' and it then appeared that, though about 2,000 tons *heavier* than the heaviest of the battleships we had previously built, she drew about two feet *less*.

Her superiority as a seagoing ship is undisputed.

If, Sir William Crookes, you will exert yourself to think a little over that, you cannot fail to recognise that every battleship we had in commission just before the 'Dreadnought' was designed constitutes a practical demonstration to the effect that the so-called First Law of Motion is not true to nature. How much longer will you and other survivors of our generation in science allow yourselves to appear to continue to be supporters of that glaring error?

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

APPENDIX I. Note to Page 7

M V^2 as centrifugal force in planetary orbits

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers

21st November, 1903.

Dear Sir,—Though the object of my letter of 3rd July was merely to support my argument to the effect that the figure 32·2 used in all engineering text-books as the Foot-Pound-Second unit of gravity is erroneous and creates unnecessary complications in engineering formulæ, and that the true figure is 16·1; that letter constitutes (by pointing out the interaction of the formulæ I had been using) the first announcement of one of the most startling and majestic among the discoveries that have been recorded in the history of science. It appears to me that the fact of my argument having led to the declaration that a transverse action of the Earth's gravitation created by its rotation must beyond the moon's orbit become uncontrollable by the radial force in which it originates, has appeared to the Council so incredible as to lead them to consider the whole argument unworthy of serious consideration.

Stokes and Huxley were, I am sure, thoroughly honest in their opposition to my views in the years 1866–1868, and were probably influenced even more by my repudiation

of the doctrine of the 'Indestructibility of Matter' than by my attack on the 'First Law of Motion'; but the Royal Society, which opposed me then, has now adopted that main point, and is disparaging me now in connection with my further development of that new philosophy exactly as Huxley, Stokes, and others treated the first step as too glaringly 'on the wrong track' to be for a moment entertained by anyone who understood what were accepted as fundamental principles in scientific knowledge.

The point now at issue is so thoroughly interwoven with formulæ in engineering text-books that, in duty to myself, to the Council, and to Engineering Science, I urge some further evidence in reinforcement of my argument as to 16·1 being the true foot-pound-second unit of gravity.

In letters on the theory of the PENDULUM, addressed to the Royal Society, I some time ago pointed out that the normal length for a Seconds Pendulum vibrating 180°, where gravity gives a fall of 16·1 feet per second, is 3·22 feet, because the work done by gravity on the pendulum in the second is naturally divisible into five equal parts, of which the fall through the length of the radius is one part.

I now urge on the Council the following figures:—

The radius of the moon's orbit is 1,261,000,000 feet, which ÷ 3·22 (normal radius of Seconds Pendulum) = 391,600,000; and the Square Root of the latter figure gives the moon's corresponding period as 19,790. That figure × 60 (the moon's distance in units of the Earth's radius) = 1,187,400; which, divided into the length of 180° of the moon's orbit (3,959,500,000 feet) = 3,334 feet per second:—which is in fact the moon's velocity.[1] I wish,

[1] The velocity of 3,334 feet per second agrees with that of the pendulum, because it is that which would result in the moon's orbit from the action of

first, to urge that the above connecting link between the radius of the pendulum and that of the moon's orbit shows 16·1 to be the true foot-pound-second unit of gravity. The fall along the radius is $\frac{1}{5}$ of the 16·1 feet of work done by gravitation on the pendulum in the second; but as the moon falls only ·0044 of a foot per second, the velocity of 3,334 feet per second along the orbit practically represents the work done on the moon per second, though that fraction of a foot is theoretically included as part of the work done.

And, secondly, I beg to draw special attention to the above use of the figure 60 as a multiplier instead of 3,600 ($= 60^2$).

The force of gravity is 3,600 times greater where acting on the pendulum than in the moon's orbit, and the use of that figure is demanded in any full theoretical statement. But the figure 60 gives a short cut to the true velocity in the moon's orbit because the error caused by its use is balanced by an equal error caused by the omission of the action of astral gravitation in the moon's orbit, constituting a resistance as the square of the velocity of motion.

The full theoretical statement of the case is as follows :—

The relative periods for the Seconds Pendulum and the moon (as Square Roots of radius) are 1 and 19,790 seconds respectively × inversely by the force of gravity in the respective positions (as 1 is to 3,600), making the relative periods 1 second and 71,244,000 seconds respectively.

The latter figure divided into 3,959,500,000 feet

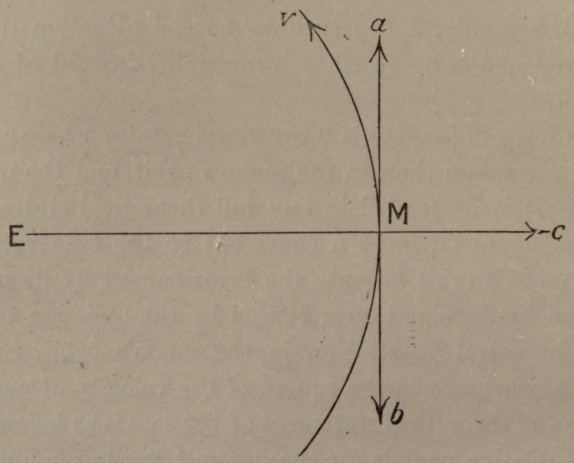
a transverse force equal to gravity. But as the transverse force acting on the moon in fact exceeds gravity as 146 is to 100, giving a transverse velocity exceeding radial velocity, 121 is to 100, the absolute velocity of the moon along its orbit round the common centre of gravity of the earth and moon is 3,704 feet per second.

The velocity of 3,334 feet per second which is at present treated in all astronomical works as the moon's *orbital* velocity, is in fact the velocity measured along the chord of an arc of 90° of its orbit.

(= 180° in the moon's orbit) gives 55·58 feet per second as the velocity which would counterpoise gravity in the moon's orbit if the vis viva of motion were not restrained by the reaction of astral gravitation against it.

If the earth and moon were a solitary system, the tangental force, M *a*, of a velocity of 55·58 feet per second, M V, would counterpoise the reciprocal force of gravitation E M.

But the reaction, M *b*, of astral gravitation against the



motion enables gravity to control a greater velocity than it could otherwise do ; and I have elsewhere shown that by that action the controllable velocity is increased in proportion as the distance is increased ; so that, instead of the normal velocity of 55·58 feet, the velocity requisite to counterpoise gravity in the moon's orbit becomes 55·58 × 60 = 3,334 feet per second.

That lifting of velocities above their normal distance from the sources of any transverse action of gravitation is the foundation of the startling law which I announced in my letter of 3rd July last, to the effect that some distance

beyond the orbit of Neptune the transverse action of the sun's gravitation becomes uncontrollable by the radial force in which it originates.

The reciprocal force, E M, between the earth and moon, actually does counterpoise the force M c ; and if the velocity were not lifted, by reaction, above its normal distance, then, to whatever extent the reciprocal radial force might be decreased by increase of distance, there would remain the same relative quantity to represent the square of the orbital velocity ; and the latter would, therefore, be equally controlled at all distances.[1]

Be the realities of the ratios in that vast interaction of forces, however, what they may, the figures which connect the work done by gravitation on the pendulum with that done on the moon must, I think, be admitted as a proof that 16·1 is the true Foot-Pound-Second unit of Force in Gravitation measure.

18th January, 1906.

I beg that you will kindly bring before the Council of our Institution the following set of formulæ for calculations connected with gravity, in correction of an absolute error which at present pervades all engineering text-books and necessitates unnecessary complications in formulæ for gravity.

[1] In a railway train the relative amounts of centrifugal force on curves of different radii are as $\frac{v^2}{r}$, as in the experiments alluded to in my letter of 30th June, 1905 (see p. 15).

In a paper on 'The Scrapping of the First Law of Motion' which I have presented to the Institution of Civil Engineers, I have pointed out that, in Astronomical phenomena such as the earth's orbital motion, the velocity is due to a combination of the tangental and centripetal forces ; whereas on the railway curve the centripetal force reduces, instead of increasing, the tangental velocity.

'Molesworth's Pocket Book of Engineering Formulæ,' p. 362, 23rd edition, takes :—

N = Number of seconds ;
S = Space fallen through in feet ;
V = Velocity in feet per second, acquired in N seconds or S space ;

and on those data gives the following formulæ :—

$$V = N \times 32\cdot2$$
$$V = \sqrt{S \times 64\cdot4} = 8\cdot025\sqrt{S}$$
$$S = N^2 \times 16\cdot1$$

Formulæ are thus given for only three out of ten different cases for which a formula may be wanted.

To get a complete systematic set of formulæ with g as the basis of them all, it is necessary to substitute *mean velocity* for 'velocity' in the above data, and then we get the following :—

$$V = N \times g$$
$$S = N^2 \times g$$
$$S = V^2 \div g$$
$$N = V \div g$$
$$N = \sqrt{S \div g}$$
$$V = \sqrt{S \times g}$$

Adding to Molesworth's above data :—

W = weight in pounds,
M = momentum, and
K = Vis Viva in units of force = 1 pound weight ;

then :—

$$M = N \times g \times W \times 2$$
$$M = \sqrt{S \times g} \times W \times 2$$
$$K = (N \times g \times W \times 2)^2 \quad \Big\} *$$
$$K = (\sqrt{S \times g} \times W \times 2)^2 \quad \Big\}$$

* or, $K = M^2$.

The error which has to be corrected in order to get a complete set of formulæ with the same figures systematically representing gravity, is to be found on Molesworth's page 363, where it is said that :—

'g = force of gravity, feet per second.
'g = 32·1889 at London at the level of the sea.'

The fact is that if g = the force of gravity in feet per second, then g (instead of 32·1889) = 16·094445 at London at the level of the sea ; or, for all ordinary calculations, 16·1 is a sufficiently close approximation, and correctly represents g in each of the above ten formulæ.

The fact that Molesworth has to change his figures in each of his three formulæ shows that he starts on a false basis by taking g = 32·2.[1]

I am, dear Sir,
Yours truly,
Wm. Leighton Jordan.

[1] The origin of this error is pointed out in my 'Essays on the Action of Astral Gravitation in Natural Phenomena,' published in the year 1900.

APPENDIX II. Note to Page 14

THE SLING

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers

10th November, 1906.

Dear Sir,—Every Engineering Text-Book I have met with agrees in asserting, or taking it for granted, that a railway train leaving a curve under the action of centrifugal force tends to follow a tangent to the curve.

The Salisbury accident appears, however, to have led to a new consideration of the subject ; as, in connection with it, the Editor of the *Engineer* says there is a ' very commonly held view that when a body flies outwards under the influence of centrifugal force, it moves radially away from the centre of gravity.' (5th October, 1906.)

I never before heard such an effect as above mentioned suggested in connection with the motion of a railway train.

What I have argued in connection with the subject is that D'Alembert's so-called fictitious force $\left(\frac{v^2}{r}\right.$ acting radially outwards) is not a ' fictitious,' but a real force.

That is, however, only one component of the force acting on the train as it leaves the rails ; and I have suggested that the combination of that force with the vis viva of tangental momentum tends to carry the train away from the curve at

an angle of about 41° (40·724 +) from the tangent (49·276 − from the radius vector at the moment of deviation).[1]

I have submitted to you the result of experiments which appear roughly to confirm that theoretical action. The question arose incidentally in connection with my demonstration of the fact that Engineering Text-Books are all wrong in supposing 32·2 to be the Foot-Pound-Second equivalent of gravity. That demonstration (commenced in the work I published in the year 1900 and culminating in the letter I addressed to you on the 18th January last) entitles the collateral suggestions I have made to respectful consideration by serious men holding influential and responsible public positions in the Engineering profession.

The published proceedings of our Institution abound with

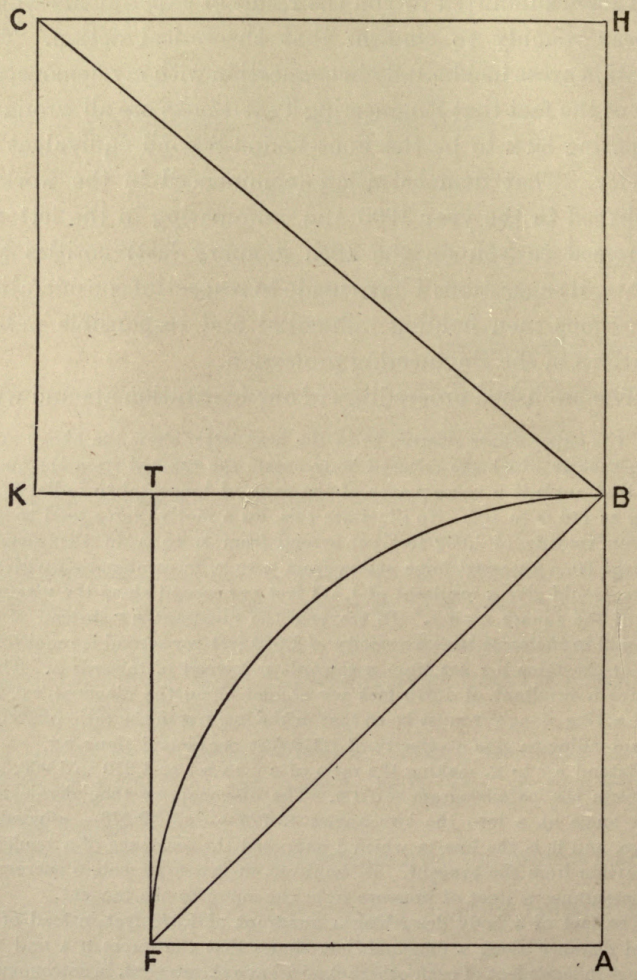
[1] The experiments referred to in the next letter show the above angle to be that at which the released body enters the tangent from the curve, and that angle is a consequence of the ratio of tangental to radial force, being as 146 is to 100. To illustrate this, let a motive force tend to give a mean velocity of 1,000 feet per second from B to A in the diagram on page 60. An equal force acting from B to T, in combination with the above, would give a resultant of 1,414 feet per second along the diagonal, B F, of the square B T F A. To traverse the quadrant B F instead of the diagonal in the same time a velocity of 1,570 feet per second is requisite.

Let the force B A act from B towards H instead of towards A. Then, to give a resultant of 1,570 feet per second along the diagonal B C, the force acting along B T must be to that in the line B H in the ratio of 146 to 100; tending to give a velocity of 1,210 feet per second along B T.

Extend B T to K, making the ratio of B K to B T as 1,210 to 1,000, and complete the parallelogram K C H B. The diagonal C B then divides the right angle at B into the two angles 40·724 + and 49·276 − alluded to above, and it is the line in which I supposed the existence of a tendency to deviate from the tangent. It, however, in fact represents a succession of instantaneous lines of pressure from the curve to the tangent.

The fact of a body describing a quadrant of 1,570 feet instead of an equal distance along a diagonal line, shows that the forces B A and B K are not independent of each other; and I have shown that, in astronomical phenomena, whereas a direct action of gravitation at B from A is inversely as the *square* of the distance, any transverse action the same force may exert is inversely as the *cube* of the distance. And, therefore, any tendency to deviate outwards from the circle is corrected by a *relative decrease* of transverse action; and any tendency to deviate inwards is corrected by a *relative increase* of transverse action.

cases in which diametrically opposite views are expressed in argument, and, therefore, in just so many downright blunders



which have helped to show the way to truth ; and I appeal to the Council to have the above angle of 41° openly tested by more reliable experiments than I have been able to arrange.

The fact that 16·1 (not 32·2) is the Foot-Pound-Second equivalent of gravity is no longer open to reasonable dispute; and if practical experiment should show that I am mistaken in supposing that the train tends to spring away at an angle of 41° from the tangent, that mistake will remain overshadowed by my correction of the recently standing error as regards the equivalent of gravity.

Major Pringle, in his report on the Salisbury accident, says nothing as to any evidence bearing on the above question: perhaps because he did not consider the question appropriate for his report: perhaps because he considers that every well-educated engineer *knows* perfectly well that the train does in fact tend to deviate along the tangent, and that it would be useless to discuss the complications of evidence in that case. By having a simple experiment arranged on a Whirling Table, the Council can for ever set at rest the difference of opinion alluded to, as above stated, by the Editor of the *Engineer*.

14th December, 1906.

Ever since replying, on 16th November, to your kind note of 14th *idem* I have been engaged in preparing a paper on the so-called First Law of Motion. I find it, however, expedient, as a preliminary step, to ask indulgence to allow me to correct an error in a letter I addressed to you on 25th October, 1905.

The preparation of my paper made it essential for me to make some mechanical experiments for the purpose of testing the sling action dealt with in my above-mentioned letter; and I find that I had misunderstood the rough experiments mentioned in that letter. In the ordinary sling the plummet, on being released, follows the tangent to the curve in which it had been revolved. To get a deviation outwards from that tangent it is necessary to have a solid radius

instead of a string, and to release the plummet, not at the periphery of the circle, but anywhere between that and the centre. As the distance of release from the periphery is increased, the angle of deviation from the tangent increases up to a maximum of about 59°.

In the ocean the water arrives with more or less of radial momentum at the successive circles it crosses, and must therefore tend to spring from the tangent in the manner shown by Diagram IV. of my above-mentioned letter. The fact that more than merely tangental momentum seemed requisite to account for some phenomena of oceanic circulation forced itself on me many years ago ; but I have never, until making my recent experiments, realised the simple character of that action, though it is virtually explained in detail in papers which I submitted to the Council of the Royal Geographical Society in the year 1876. And also, in the 'Treatise on Vis Inertiæ,' which I published in the year 1868, I commented on the intrinsic difference between the action of forces in ordinary sling experiments and that of forces acting on drops of water in the ocean and in astronomical phenomena. Those views are confirmed by my recent experiments, and make my recent mistake regarding the ordinary sling doubly strange.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

APPENDIX III. Note to Page 20

THE MOON'S PERIOD OF REVOLUTION

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers

13th November, 1905.

Dear Sir,—In my letter of 6th instant I turned aside from the majestic subject of Stellar Time to that of your letter of 4th ultimo, which required attention ; and I now revert to the former for the purpose of commenting on a modification of hitherto accepted ideas as regards lunar and planetary velocities of orbital motion of such magnitude as glaringly to demand explanation.

The period of 24·38 days, which I have given as that in which the moon completes an orbital revolution, requires (the radius of the orbit being $1{,}245 \times 10^6$ feet) a velocity of motion along the line of the orbit at the rate of 3,714 feet per second, whereas astronomers have been agreed in treating the actual velocity measured from star to star as only 3,334.

That apparent discrepancy of 380 feet per second is approximately due to the fact that astronomers measure the velocity *not* along the line of the orbit actually traversed by the moon, but along successive chords.

As Sir Isaac Newton's demonstration of the action of the earth's gravity in the moon's orbit is the keystone of Natural Philosophy, when extended beyond terrestrial regions, I give

the following statement to show the approximate agreement of his figures with the period of 24·38 days which I have given for the moon's orbital revolution.

Chambers gives 3,334 feet per second as the moon's orbital velocity, which (though not stated to be so in astronomical works) is the distance measured from star to star along the chord of the arc traversed by the moon ; and not along the orbit, which is the moon's true path.

That figure, 3,334, being the mean velocity along the chord, makes the corresponding mean velocity along the radius of the orbit 3,334 divided by 1·414 = 2,357·8 feet per second.

Newton, in the ' Principia,' gave for the moon's fall from the tangent to its orbit, 15 feet 1 inch $1\frac{4}{9}$ lines per minute in old French measure ; which I make approximately equivalent to ·00447 of an English foot per second.

That figure, ·00447, divided into 2,357·8 as the mean velocity of fall along the radius in an arc of 90°, obtained as above from Chambers's orbital velocity of 3,334 feet per second, gives 527,472 seconds as the time in which the moon falls the length of the radius of its orbit ; and, therefore, makes the period in which it completes an orbital revolution 527,472 × 4 = 2,109,888 seconds, or 24·42 days. The discrepancy between 24·38 and 24·42 is partly due to the difference in the estimates of the moon's distance, which were accepted by Newton and by Chambers respectively.[1]

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

[1] A more definite estimate is given in my letter of 3rd May, 1906, on page 34, but is subject to slight corrections.

APPENDIX IV. Note to Page 26

MR. FRANCIS DARWIN ON 'THE UPWARD FLOW OF SAP IN PLANTS'

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers

4th February, 1905.

Dear Sir,—Fluid motion has been so much before our Institution in recent discussions as to make it specially appropriate for me, in my endeavour to induce our Council to rescue the Royal Society from its bondage to obsolete notions on the subject, to refer to a lecture on 'Blood Pressure in Man,' delivered in The Royal Institution last evening.

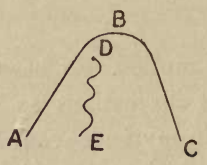
In the course of his lucid and very interesting treatment of the subject, the lecturer described an enormous frictional resistance to the circulation in the arteries and veins just as a practical engineer would describe the resistance to the flow of water through a pipe under pressure from a hydraulic ram, and commented on the great contractile power of the heart requisite to overcome such resistance.

I do not, in the absolute manner in which I deny the existence of the Great Southern Ocean tide which is supposed to be the origin of the tides all over the world, deny that the heart can exert the supposed amount of pressure; but wish to show reason for considering that the motive force which keeps up the circulation acts locally throughout the course

of circulation requiring the heart to exert force proportional only to the relative quantity of the blood it may contain ; and if called on for more than that exertion, it is then struggling against a diseased condition of the system.

Let A B C be a mountain with a river, D E, running down its side from a spring at D.

The water is raised, within the mountain, to D by atomic or molecular gravitation preponderating over gravity. But when the water reaches the orifice at D, gravity preponderates and carries the water downwards ; the molecular gravitation over which gravity preponderates being represented by the friction which checks the downward motion.



It seems to me that the circulation of the blood must be assisted by a similar action of molecular gravitation between the molecules of the blood and the sides of the narrowing arteries ; and in the widening veins the capillary action is diminished, leaving the molecular gravitation of the blood itself to draw it together and into the heart, where only it can aggregate, just as moisture on a leaf is drawn into the formation of a dewdrop in opposition to the action of gravity. Molecular gravitation appears to be a motive force equally distributed throughout the circulation in conditions of health, and the question at issue is as to the ratio it bears to the hydraulic ram or ' head of water ' pressure caused by the contractile action of the heart.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

PART II

LETTERS IN CONNECTION WITH LECTURES DELIVERED IN THE ROYAL INSTITUTION AND IN THE INSTITUTION OF CIVIL ENGINEERS

JUNE 1907 TO JULY 1908

THIS VOLUME IS DEDICATED

TO

SWINFEN KER JORDAN, ESQ.

OF

BUENOS AYRES

AND TO THE

ANONYMOUS

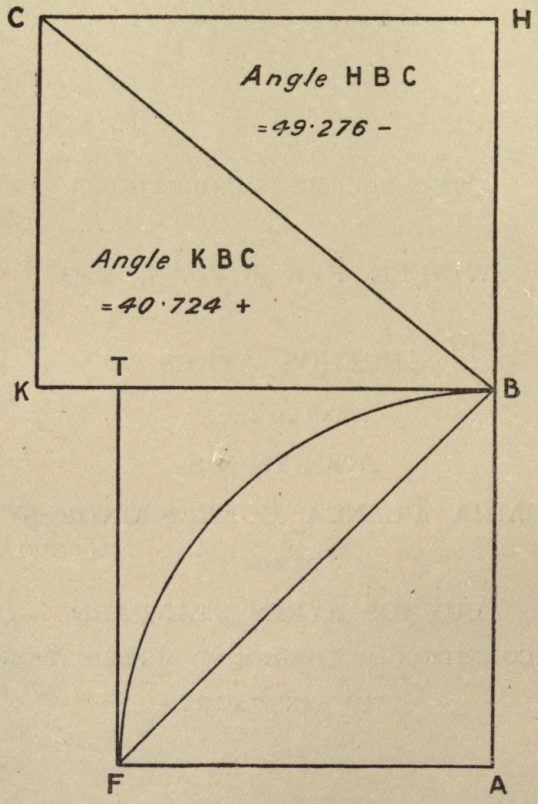
BAHIA BLANCA CORRESPONDENT

OF THE

BUENOS AYRES *STANDARD*

IN RECOGNITION OF THEIR KIND APPRECIATION OF

THE FIRST PART



THE SLING FORCES

The ratio of the tangental force, B K^2, to the radial force, B A^2, is nearly $14\frac{2}{3}$ to 10. If arithmetic could give that ratio accurately it would also give the ratio of the circumference of the circle to the radius.

PREFACE TO PART II

As a connecting link between this Volume and its first part I have reproduced, as a Frontispiece, a diagram showing the ratio of tangental to centripetal force in all circular motion.

A B T F being a square, $A B^2 + B T^2$ is equal to the diagonal $B F^2$.

Also, the side B H of the parallelogram B H C K being equal to A B and in the same straight line, then $A B^2 + B K^2$ is equal to the diagonal $B C^2$, whatever may be the length of B K.

Let a motive force tend to give a mean velocity of 1,000 feet per second from B to A. An equal force acting from B to T in combination with the above would give a resultant of 1,414 feet per second along the diagonal B F of the square B T F A. To traverse the quadrant B F instead of the diagonal in the same time, a velocity of 1,570 feet per second is requisite.

Let the force B A act from B towards H instead of towards A. Then, to give a resultant of 1,570 feet per second along the diagonal B C, the force acting along B T must be to that in the line B H in the ratio of 146 to 100; tending to give a velocity of 1,210 feet per second along B T.

Extend B T to K, making the ratio of B K to B T as 1210 to 1000, and complete the parallelogram K C H B. Then the quadrant B F is equal to the diagonal B C, because they are resultants of similar forces, $B K^2 + B H^2$ and

B K^2 + A B^2. And the sides of the square described on B C are together equal to the circumference of the circle of which the curve line B F is a quadrant.

The moon, and each planet, follows the curve B F, because the transverse force B K^2 in its orbit is a function of the centripetal force B A^2, decreasing as the *cube* of the distance from A increases, whilst the centripetal force itself decreases as the *square*; and, therefore, any tendency to deviate outwards from the circle is corrected by a relative decrease of transverse action, and any tendency to deviate inwards is corrected by a relative decrease of centripetal action. The action of each perturbing force in the orbit is either corrected, or else given permanently a natural limit in that same manner.

Arithmetical numbers will not express precisely the above ratio of forces—the magnitudes at which the two angles (together 90°) specified in the diagram make the diagonal B C exactly equal to the quadrant B F—or the ratio which those two equal lines bear to the two equal lines B H and B A; and all the figures given above are therefore merely the approximations given by integral numbers on the basis of 1000 representing a side of the square A B T F.

ROYAL SOCIETIES CLUB,
August, 1908.

LETTERS

To Sir William Crookes, D.Sc., F.R.S., Secretary, the Royal Institution

24th April, 1908.

Dear Sir,—In a letter which I addressed to you on 8th June 1907, I endeavoured to interest the Royal Institution in the announcement I was just then enabled to make of a discovery which I had long ago vaguely imagined, notwithstanding what seemed to be an insuperable barrier of plausible and generally accepted argument which had appeared to be at variance with it. In that letter I limited myself to the declaration that the vibration theory of light which had long been accepted in the scientific world had unconsciously been exploded by Sir James Dewar in the course of his lecture of the previous evening.

The idea that light requires the assistance of Æther, 'the ether,' or that of any substance to enable it to vibrate may be relegated to the limbo of exploded fancies. Though light originates in transmutation from material substance, it is, when once created, an independent immaterial force which emanates in all directions from the source of its creation; and it vibrates, without extraneous assistance, in its own natural course of filling space, expanding as the cube of the distance traversed; having, therefore, as much, or more, lateral as direct motion. It requires no help from any material substance to enable it to follow its course. Only in empty space it has perfect freedom of action, and it is

merely more or less hampered, distorted, or spent by encountering any substance, however attenuated.

The abolition of the idea of any material substance being necessarily distributed throughout interplanetary space to account for the vibration of light constitutes a step of far-reaching importance in natural philosophy. In the action of gravitation there is no such lateral clashing of the force.

The force of gravitation which is in fact exerted by every material particle would be non-existent outside any particle but for the existence of surrounding particles, with each one of which gravitation creates for it a reciprocal bond of union.

The action of gravitation is in lines of force extending from every material particle to every other particle, and differs in that respect from the action of any such independent force as light, as it does not spread itself for the purpose of filling space, but exists only where some material particle exists to reciprocate the action of other particles. Light, on the contrary, is independent of such reciprocal action, and spreads itself to fill its sphere of action without requiring a reciprocal bond of union to give it existence at any point within that sphere.

Perhaps you will not fully appreciate the importance of that distinction without knowledge of what I published on the ' creation of force ' forty years ago. The arguments I then used have in fact led to the elucidation of this practical difference between the modes of action by which light and gravitation are respectively distributed around any centre from which they may respectively act.

That difference may appear superficially to be a trivial distinction, but it is in fact a point of most profound and far-reaching importance in the elementary principles of natural philosophy.

6th May, 1908.

The 'Scientific Work of Lord Kelvin' must, I think, be considered by all members of the Institution as the most appropriate subject with which the current session of evening meetings could have been commenced; and the manner in which it was dealt with by Dr. Larmor last Friday was certainly of intense interest to me as one item in the Lecture Hall where all were keen to hear the achievements of the most renowned of our generation of scientific men officially summarised by the Secretary of the Royal Society.

Lord Kelvin may fairly be regarded as a central figure in a half-century which may perhaps hereafter take its place in history as the most remarkable transition period which the scientific world has hitherto experienced. It commences with the work of John Dalton, well established for its basis, and closes with the startling discovery of the action of radium; and, as the dust of atomic theories has to some extent led to a confused treatment of fundamental principles of natural philosophy, I think it expedient to press on the Royal Institution some important points which have not been recorded by Dr. Larmor.

Lord Kelvin was in his younger days author, jointly with Professor P. G. Tait, of a treatise on Natural Philosophy, which soon became the most generally received text-book on the subject. In the preface to that treatise, dated July 1867, the authors say:—

'We give Newton's "Laws of Motion" in his own words, and with some of his own commentaries—every attempt that has yet been made to supersede them having ended in utter failure. Perhaps nothing so simple, and at the same time so comprehensive, has ever been given as the foundation of a system in any of the sciences.'

I have been specially interested in the opinion then held

by Lord Kelvin and Professor Tait, because in 'An Investigation of the Forces which determine the Position and Movements of the Ocean and Atmosphere,' which I had published in the year 1866, I had substituted for Newton's 'First Law of Motion' a new definition of his 'Centrifugal Force,' and the above extract from the first edition of the treatise by Thomson and Tait showed their rejection of my new view regarding the action of astral gravitation.

It is only because (as just above suggested) in the dust of atomic theories broad principles have been almost lost sight of, leading scientific men have remained unconscious of the fact that the opinion so clearly and so forcibly expressed by Thomson and Tait in 1867 has been practically abandoned throughout the scientific world. That change is made evident by the article 'Mechanics,' in the ninth edition of the 'Encyclopædia Britannica,' published in 1883. Professor Tait there says:—

'The first law is superfluous, because its consequences are all implied (by negation) in the statement of the second.'

That shows a decided change of front between the year 1867 and the year 1883 as regards the merits of Newton's mode of stating the Laws of Motion. I must, however, frankly say that Professor Tait's statement to the effect that Newton's First Law is implied in his Second Law passes my comprehension; and I leave its explanation to anyone who finds himself able to understand it. All I know is that my substitution of a transverse action of Newton's law of gravitation to do all the work in the solar system for which he supposed his first law of motion to be requisite does not invalidate his second and third laws. Newton's law of gravitation supplies a transverse action in each planetary orbit, the ratio of which to the radial action is, in each orbit, as 146 is to 100 approximately; and Tait is therefore quite right in saying that the first law is superfluous; but I find

no reasonable foundation for the idea of its being implied in the second law.

The 'Astronomical Register' in August 1874 concluded a review of one of my books with the words: *Nolumus leges motus mutari.* What have they—and you, Sir William Crookes—to say to the ninth edition of the 'Encyclopædia Britannica'? Tait was chosen to deal with the subject in that publication as being, with Lord Kelvin behind him, the most generally accepted authority. Do you still stand in the position held by the 'Astronomical Register' thirty-four years ago, blind to its unfair persistence in hostility to astral gravitation?

8th May, 1908.

The second point contemplated in my letter of 6th instant is created by Dr. Larmor's comments on the recent application of gyroscopic action by Dr. Schlick to the prevention of the rolling motion of ships at sea.

Dr. Schlick's work was alluded to after an interesting account of Lord Kelvin's gyroscopic experiments as if the action had been understood and its application as contrived by Dr. Schlick in fact foreseen by Lord Kelvin. Dr. Larmor's statements to the above effect show a thorough misappreciation of what has been done by Dr. Schlick. In describing Lord Kelvin's work Dr. Larmor did not exhibit or allude to any experiment bearing in any manner on the main principle involved in Dr. Schlick's work. If either the theoretical ideas or their practical application had ever before been described I could scarcely have escaped hearing something of them; in fact, however, I went to Dr. Wurl's recent lecture on the subject at the Society of Arts puzzled by what I had previously heard, and I left the Lecture Hall more bewildered than I had entered it. But before I had walked for many minutes along the Strand, thinking over

what I had seen, I grasped the simple idea involved and its clever application.

It has now for many generations been a commonplace proceeding among civilised people to arrange a weight in a grandfather's clock in such a manner that, instead of falling freely so as to give with half a second's work a heavy blow, a brake is applied to the motion so as to equally distribute the force over seven or eight days instead of allowing it to expend itself in half a second ; but it seems to me that until after Dr. Schlick had not only conceived the idea of putting a brake on the instantaneous gyroscopic action which if left free would increase the rolling which brought it into play, but had also made it experimentally effective, neither Lord Kelvin nor any other scientist had done anything in the matter either in theory or in practice ; at any rate, Dr. Larmor showed no experiment touching what seems to me to be Dr. Schlick's completely new invention.

Thomson and Tait's ideas at the date of the experiments and writings dealt with by Dr. Larmor are now quite obsolete as regards gyroscopic action ; they neither of them then understood its fundamental principles, and I have seen no indication of either of them having done so since. Be that, however, as it may, I am sure Lord Kelvin would repudiate the idea of Dr. Schlick's special discovery having been anticipated by him, and his renown is great enough without taking what belongs to other men.

23rd May, 1908.

' He did not say what the Ether is, did he ? '

' No ; *that* he did *not* tell us.'

The above words I chanced to overhear just before last night's lecture at the Royal Institution, and have no doubt they referred to Professor Trouton's very interesting lectures entitled, ' Why Light is Believed to be

a Vibration' (12th instant), and 'What it is which Vibrates' (19th instant).

I watched carefully as Professor Trouton repeated one by one all the most important of the experiments which the marvellous resources of the Institution have accumulated for the purpose of analysing the action of light; and after the first lecture it seemed to me that the lecturer could scarcely any longer imagine that light requires the assistance of any material substance to enable it to travel from the source of its creation. I was therefore disappointed when the only conclusion arrived at was that the *same ether* which propagates light by its vibration, suffices also to account for electrical action; but I found two consolations —one in the fact that the corpuscular theory was ignored as if no longer running as a competitor for the explanation of light; and the other in the fact that *one* ether only is now wanted by science, instead of two, or three, or four, or five, as have from time to time been theoretically created, each, for its own purpose, filling the universe and playing its own game in every point of space regardless of opposite actions at the same points by its co-existent ethers.

I did not grasp any intrinsic divergence by Professor Trouton from Maxwell's electro-magnetic theory of light, but his general method of dealing with the subject seemed to me to constitute a step in the right direction as facilitating a transition to the idea that light spreads, and vibrates as it does so, by its own inherent action; and that material substances all more or less thwart or distort its course; variously separating, transmitting, reflecting and deflecting its rays.

You know, Sir William Crookes, that Science once kept a tortoise to carry the earth and keep it from tumbling nobody knew where; is the Royal Institution to persist in supposing that light requires 'the ether' to carry it any more than the earth requires the tortoise?

The above is in some respects a suitable prelude to last night's lecture. It was a treat to follow the masterly manner in which the lecturer handled his majestic subject and arrived at his provisional estimate of the diameter of the material universe as 30,000 light years.

In the course of the lecture I reflected that it is now much more than forty years since I first read the lemmas prefixed to Newton's 'Principia,' and put them in quarantine; more especially Lemma II, as the keystone of their argument; and a few years ago a matter which struck me as clearly at variance with that argument led me to look at it again, with the result that though I did not feel strong enough in mathematics, or in possession of leisure enough, to make a formal and open attack on it, I framed a proposition directly at variance with it which I addressed to the Royal Society on the ——.

I limit myself for the present to suggesting that the above-mentioned Lemma of Newton's is the basis of the persistence in an error arrived at by measuring the velocities in planetary orbits along chords of arcs from point to point instead of along the arcs actually traversed.[1] And also that the reality of the absolute velocities of motion along the arcs is a strong corroboration of the opinion arrived at by Professor Kapteyn to the effect that the material universe is of a definitely limited extent.

My attention has been so much distracted from Professor Larmor's lecture that I fear some points on which I hoped to comment have slipped my memory, at least for the moment. I must, however, say that I have looked at the paper by Lord Kelvin in the 'Philosophical Magazine,' published since his death, and referred to by Professor

[1] See footnote, Part I., page 52. The exposition of The Sling Forces given in the Preface to Part II. obliterates the *raison d'être* of Newton's Infinitesimal Analysis.

Larmor. The point in the paper which struck me as of the greatest interest is the manner in which it concludes with a quotation, given in inverted commas to show it to be *verbatim*, from a lecture delivered by Lord Kelvin himself in the year 1854, making a suggestion to the effect that gravitation may be the antecedent of all the motion, heat, and light at present in the universe.

I felt specially interested in that last point touched on by Lord Kelvin in his final effort in the cause of science, because in the year 1866, when I published my first work, I supposed that idea to have been originated by Grove in his 'Correlation of Physical Forces,' to which I alluded in the Preface of the work published that year, and also in the Appendix of another work I published in 1868. The bringing forward of the quotation now, just fifty-four years after its publication, is also of still greater and general interest as showing the vast importance attached by Lord Kelvin to the idea he then expressed, which has certainly developed to an extent and in a manner unthought of by either Kelvin or Grove when they were its bold and far-sighted pioneers.

13th June, 1908.

On the 3rd June, 1904, you gave the members of the Royal Institution the pleasure of hearing Professor Svante Arrhenius lecture on 'Electrolytic Dissociation.'

He puzzled me on the main point notwithstanding all his interesting detail.

He began by telling us that modern science has reached the same conclusion as Democritus, who taught that 'bodies are built up of indefinitely small indivisible particles which he called atoms.' And he further told us that Newton accepted the atomic theory of Democritus.

It seems to me that there is nothing in the history

of science that can be more certainly relied on than that Arrhenius is right in saying that Newton and Democritus fully accorded in considering material atoms to be indivisible particles. And if modern science stands with Democritus and Newton on that fundamental point, its position is very clearly defined by both those philosophers in unambiguous language. But, after all that, the lecturer concluded by virtually adopting the idea of matter being only a manifestation of 'electro-dynamic forces,' and declared that 'the bold previsions of Sir William Crookes seem to be rapidly acquiring a concrete form, to the great benefit of scientific evolution.'

I do not know what the bold previsions alluded to are. But it seems to me that the simplest of common sense suffices to make it evident that the idea of matter being merely a manifestation of 'electro-dynamic forces' is fundamentally at variance with the ideas of Democritus and of Newton as to the eternal indivisibility of the atom. Modern science either considers every atom of the material universe to have existed from eternity and to be for ever indivisible, or else it has abandoned the ideas of Democritus on that question. What, in connection with electrolytic dissociation, is there that is bold in the previsions of Sir William Crookes if they do not constitute some advance from Newton's idea that material atoms were 'in the first creation made so hard that they can never be broken in pieces'?

Is the electro-dynamic theory alluded to by Professor Arrhenius, and which I must suppose to be clearly understood by him and by its inventors, most in accordance with that idea of Newton's or with the diametrically opposite idea which I have propounded to the effect that in the transformation of water to steam by the action of heat the atoms which existed in the water are not merely *scattered*, but are each one individually rent asunder and transformed in

a manner utterly at variance with the ideas held by Democritus and by Newton under any reasonably common-sense interpretation of the language used by either of those philosophers ?

Where does Arrhenius place modern science now ?

You, Sir William Crookes, must surely know what he means by his statements. Is modern science with me as regards the transformation of the atom, by dissociation of its constituent parts, as water is converted into steam by the application of heat ? If so, then it has absolutely abandoned the atomic theory held by Democritus and by Newton.

Another puzzle in connection with this question is that if atomic dissociation results from mere change of temperature whenever any cook boils a kettle, why should that be called ' electrolytic dissociation ' ? The dissociation has no apparent connection with any electro-dynamic theory.

What has led me back to the very interesting address delivered by Arrhenius four years ago in the Royal Institution is his book, ' Worlds in the Making,' which has been published this year in London.

In that work Professor Arrhenius gives a description of the experiment shown in the Royal Institution by Professor Nichols, in May, 1905.[1]

The initial point of the argument used by Professor Nichols seemed to me at the time to be, that, as the force of radiation pressure acting on any body is as the square of the diameter, and the resistance to the pressure as the cube, the smaller of any two bodies will be more easily moved by a given force of pressure than the larger body ; but, in fact, in the experiment the bodies which we saw pushed away from the light whilst others mixed with them fell vertically, had thirty times less specific gravity than the later ; and therefore the argument was not rigidly illustrated by the

[1] Dealt with in my letter of 15th May, 1905, Part I., page 5.

experiment. Notwithstanding that discrepancy the experiment is one of great interest and importance in connection with the action of light, which was, I think, first demonstrated by Crookes's radiometer. It was not clear, however, that the lens which focussed the light of the electric lamp used by Professor Nichols did not make his experiment an illustration of the action of heat rather than of light.

The fact of extraneous pressure against spheres being as the squares, and the resisting force of gravitation as the cubes of their diameters, seemed to me to be given as if it had only recently been recognised. It has, however, long been familiar to me in connection with the action by which grains of sand, or pebbles, are rolled along the bed of a stream. That I believe to have been long ago recognised in some engineering works, and I built upon it when more than thirty years ago I pointed out that a star exceeding the sun in size and splendour, but similar in all other respects, might be so far away as to be out of reach of the strongest of our telescopes, but, nevertheless, be exerting a force of gravitation in the solar system exceeding that of the sun ; because the force of gravitation is as the cube of the diameter and the visibility only as the square. For that same reason the pressure of a stream against round pebbles of different size is as the squares of their respective diameters, and the resistance to the action of the stress is as the cubes of the diameters ; so that, therefore, the ratio of pressure to resistance is greatest in the smallest pebbles.

I find Arrhenius rather hazy in his treatment of the sun's radiation pressure ; but his description of the changing phases of the corona is quite in accordance with the action of the forces described in a paper which I offered to the Royal Geographical Society in the year 1876. There is nothing hazy about that paper. It is a clear and explicit demonstration of the circulating action in the ocean caused

by the earth's rotation which is equally applicable to the effect of the sun's rotation on any fluid, or haze, resting on the sun ; and the recent discoveries of the movements of the sun's corona are a splendid corroboration of the theoretical action described in that paper, which covers two octavo pages, or, with a supplementary proposition, three pages.

I return to Dr. Larmor's lecture of 1st May because I strongly object to Newton being claimed by the Etherites as belonging to their school. Dr. Larmor referred us to a paper of his in the ' Chemical News ' of 6th March, and in that paper he alludes to Newton as having dealt with the structural connection of atoms with the ether, and specially alludes to an ' atomic theory ' given in his ' Opticks ' ; what I find in the ' Opticks ' is Newton's explicit statement that, ' It is manifest that the heavens are void of all sensible resistance and by consequence of all sensible matter ' ; and that is the view which pervades the ' Principia ' ; being, in fact, necessitated by the main argument of that work as regards the interaction of centripetal and centrifugal force.

In the paragraph to which Dr. Larmor specially refers I find Newton says : ' Matter is not necessary in all places.' And just before that he says that matter is formed of solid, massy, hard, impenetrable, moveable particles ; and that ' the changes of corporeal things are to be placed only in the various separations, and new associations, and motions of these permanent particles.'

Tell me, Sir William Crookes, are not Arrhenius and Larmor mistaken in supposing that modern science stands with Newton as regards his ' atomic theory ' as above stated in his own words ? Is not leading opinion now more nearly in conformity with my argument to the effect that in the transformation from water to ice there is a transformation of the constituent atoms, and not merely a ' new association ' of them ?

I do not say that Dr. Larmor has no basis for connecting Newton's name with ether pressure, for I long ago noticed that when perplexed by charges of being atheistical in his idea of gravitation acting across empty space all the way from the earth to the moon, he tried to create material connection; but when so doing he forgot the principles of his own 'Principia.'

With all due deference to Arrhenius and to Larmor, I must say it seems to me that the attempt to validate Newton's atomic theory has failed as completely as the attempt to invalidate his law of gravitation.

25th June, 1908.

In a letter which I addressed to you on 11th April, 1906, I commented on an important gyroscopic experiment which Professor Thomson had exhibited in one of his lectures shortly before that date: I believe in that of 7th April.

I take the annexed two diagrams from page 124 of the 'Essays on the Action of Astral Gravitation' which I published in the year 1900, as they serve to show accurately the character of the experiment placed before us by Professor Thomson.

A rotating gyroscope suspended by a cord in the position B in figs. 1 and 2 Professor Thomson moved from the vertical, so as to give it a motion of revolution in addition to its motion of rotation.

At one time the revolution was given in the same direction as the rotation, as in fig. 1; and at another time in the opposite direction to the rotation, as in fig. 2. And he showed us that in one direction the motion of revolution was more rapid than in the other.

I did not succeed in following the argument by which Professor Thomson made the vast velocities of the vibration

motion of his infinitesimal corpuscles explain the difference in the velocity of the gyroscope's revolution under the two different conditions shown in figs. 1 and 2. I have, however, shown not only in connection with the diagrams I have reproduced here, but in many other parts of my

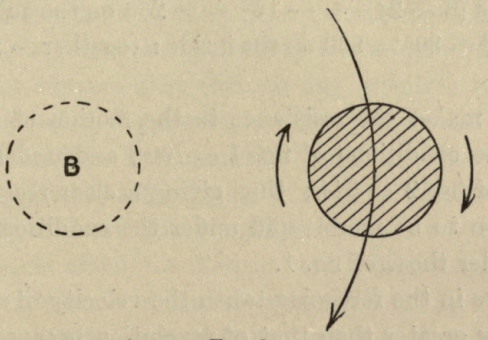


FIG. 1.

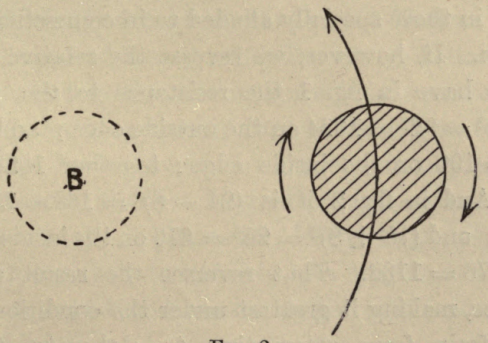


FIG. 2.

works, that under the conditions shown in fig. 2 the motion of revolution is less resisted than under the conditions shown in fig. 1 ; and I presume that to have been the case in Professor Thomson's experiment.

That is so because the resistance of astral gravitation to motion is as the square of velocity.

Let the velocity of revolution on the outside edge of the disc be 8 and on the inside edge 6. And let the rotational velocity of the periphery be 24.

Then in fig. 1 the resistance to the revolution is $(8 + 24)^2 = 32^2 = 1024$ on the outside ; and $(6 - 24)^2 = -18^2 = -324$ on the inside : together $1024 - 324 = 700$. And in fig. 2 it is $(8 - 24)^2 = -16^2 = -256$ on the outside ; and $(6 + 24)^2 = 30^2 = 900$ on the inside : together $-256 + 900 = 644$.

That makes the resistance to the motion of revolution under the conditions of fig. 1 = 700 ; and under the conditions of fig. 2 = 644 ; thus giving a clear reason for the revolution to be more rapid under the conditions of fig. 2 than under those of fig. 1.

I have in the foregoing taken the velocity of rotation as decidedly greater than that of revolution because that was clearly the case in Professor Thomson's experiment, as it also was in those specially alluded to in connection with my diagrams. If, however, we reverse the relative velocities, then we have in fig. 1 the resistance to the revolution $(24 + 8)^2 = 32^2 = 1024$ on the outside edge ; and $(18 - 8)^2 = 10^2 = 100$ on the inside edge : together $1024 + 100 = 1124$. And in fig. 2 it is $(24 - 8)^2 = 16^2 = 256$ on the outside ; and $(18 + 8)^2 = 26^2 = 676$ on the inside : together $256 + 676 = 1132$. That reverses the result as regards resistance, making it greatest under the conditions of fig. 2.

I refrain from attempting to elaborate this point further at present, because it seems to me that you have now before you Professor Thomson's experiment explained in a more clearly defined manner by the action of astral gravitation than by his corpuscular theory as expounded at the lecture to which I have alluded. *Quousque tandem*—— ?

3rd July, 1908.

As in the letters I have addressed to you it has been requisite for me in several instances to point out that the theory of astral gravitation had originated some now prevalent ideas, or discoveries, which were when first announced scouted by all leading scientific societies and journals, I read with keener interest than most men the leading article in yesterday's *Times* on the Darwin-Wallace celebration.

I cannot suppose that there is any scientific man who does not appreciate the manner in which Darwin and Wallace treated each other in connection with the discovery of 'natural selection' as being a line of conduct which redounds to the credit of the scientific world. It does not concern the scientific world alone, for it is, in fact, a bright example of the highest instincts of human nature.

But the general tenor of the article is in disparagement of any claimant in disputed questions of priority, which is a matter which did not arise in the Darwin-Wallace episode. Is it to be supposed that if either Darwin or Wallace, or the two jointly, had in the first instance been rejected by the learned societies, they would, after publishing on their own responsibility, have thought it right to stand by in silence whilst the same societies long afterwards accepted their views presented by a plagiary? Might they not perhaps have been more likely to feel it to be a duty to face discomfort, disparagement and abuse, to prevent if possible the triumph of injustice?

I presented my theory of astral gravitation to the Royal Geographical Society without any thought of publishing on my own responsibility until after it had been rejected. Would Darwin, or Wallace, have had so little confidence in their views, or so little appreciation of their value, as to have refrained from publishing on their own responsibility if the learned societies had rejected them?

We are all entitled to admire Darwin's self-control in making his discovery a secret for twenty years, without on that account failing to be in sympathy with the more impetuous course to which Wallace was impelled when his investigations in the forests of the tropics led him to the same discovery. I was neither so reticent as Darwin nor so impetuous as Wallace; thirteen years before offering my paper I had clearly recognised that the action of astral gravitation excludes the so-called First Law of Motion from the realms of practical science; and two years later a loitering voyage on the Paraná led me to recognise the action of the earth's rotation on that river in a manner which gave a clue to its action in producing the general features of the existing circulation of the ocean.

I think that according to the general tenor of the *Times* article this letter of mine is very reprehensible, and I am sure that the writing of it gives me no pleasure.

8th July, 1908.

The question is, What are these little corpuscles which you and your colleagues on our committee like better than astral gravitation?

Newton, believing light to be a material substance, wanted them as the atoms of that substance and made them dash with the velocity of nearly 200,000 miles a second from the sun to the earth; or from a tallow candle to the pages of a book read by its light.

But now (*pace* J. J. Thomson) nobody wants the little things to do such a pace as that. Young and Kelvin, following the main idea of Huyghens, have required them merely to vibrate, and become luminiferous as they vibrate, as waves are propagated through them from any source of light; and I understand that the velocity of motion which

Kelvin has recently required them to effect in their vibrations is reduced to a rate of 100 or 120 miles an hour.

Young supposed these corpuscles, or atoms of ether, not to be obliged to get out of the way of the earth as it moves through space, but to be so small as to be able to pass through the earth—that is, through the walls of our houses, through the chairs and tables in our rooms, and through our own bodies—like water through a sieve. I have heard Kelvin speak in accordance with that view, though he has more recently denied the possibility of such action, on the ground of a demonstration to the effect that the ether cannot be a fluid, as had been supposed, but must of necessity be an elastic solid, making it impossible for the atoms of the ether to pass through our bodies, like wind through a tree, without the atoms of the ether and the atoms of our bodies occupying the same space at the same time. That is professedly arrived at by argument remorselessly followed to its logical conclusion, and it might, I think, have led most men to suppose any claim of the ether to existence as a material substance to be obliterated by such a conclusion. But with Kelvin the resources of science were not exhausted, and he boldly adopts what he declared to be the only way to protect the ether, by simply denying 'the scholastic axiom that two portions of matter cannot jointly occupy the same place.'

I beg you, Sir William Crookes, rather than let go of that common sense 'scholastic axiom,' to let 'the ether' perish, and recognise that so-called ether pressure is merely the opposite aspect of astral gravitation, as argued throughout this one-sided correspondence.

Granting the conclusion arrived at by Kelvin, to the effect that the earth and the ether must occupy the same space at the same time, there is a way of accepting that conclusion without abandoning the old axiom that two

atoms of matter cannot occupy the same space at the same time.

Lord Burleigh and his nod were in the same place, and also the wisdom of which his nod was significant, because the latter was one of his attributes, and the nod his own resultant movement. And the alternative to denying the old axiom when Kelvin's argument makes the particles of the ether occupy the same space as the particles of the earth is merely to admit that the argument has reduced the ether to the status of being an attribute of matter.

Matter and inertia are identified, so that the quantity of the former in any given space is the exact measure of the latter in that same space; and gravitation is the force exerted by each material particle as connecting links extending between it and every other particle of matter. Has not Kelvin's argument in fact reduced the ether to the status of being the force of gravitation exerted at every point in which a particle of matter exists? He has located it in matter in the sense in which the force exerted by the hand is located in the hand.

A remarkable episode in connection with this question is the passage of an eminent statesman through it like a comet from the outside regions of the realms of science. In Mr. Balfour's suggestion to the effect that modern science not only explains matter, but explains it away, may be vaguely traced a distorted shadow from the doctrine of evanescence which makes material existence a passing phase linking together pre-material and post-material forces.

But if science is really with Mr. Balfour in the idea that, ' without the ether an electric theory of matter is impossible ' then the electrical basis of matter described by him is destroyed by Lord Kelvin's latest argument, except among those exceptional philosophers who are able with him to

believe that two particles of matter can be in the same space at the same time. Perhaps Lord Kelvin had before him a space of four dimensions ; and, whilst putting one particle into the three dimensions known to our school books, put the other into the fourth dimension, which is nobody knows where.

The conclusion is that the tiny corpuscles which are supposed to constitute ' the ether ' are infinitely more tiny than any etherite supposes ; which logically means that they have no material existence ; and therefore we need not impose on ourselves the idea that two material particles can jointly occupy the same space, or that space can have more than three dimensions.

24th July, 1908.

I know that I did not write the leading article in the *Times* which originated the letters which I addressed to you on 3rd and 8th instant ; and I am sure you did not write them, because you could not have done so with my correspondence of the last two years before you without truly realising the situation and then feeling it to be unworthy of the high position you hold in the scientific world to make the Royal Institution under your guidance a shelter for misappropriation of renown and the rewards which accrue to its possessors.

I recently alluded to the letters I have addressed to you as constituting a one-sided correspondence ; but they are so only in a technical sense, or from a narrow point of view. Broadly seen from a loftier point of view, your part is performed by many lecturers speaking under your secretarial wings with contorted fragments of my work as the unacknowledged basis of their discourse.

I have in some of my preceding letters pointed out that Thomson and Tait, in the first edition of their ' Treatise on

Natural Philosophy,' declared that they gave Newton's laws of motion in his own words because they considered it impossible to improve his methods of treating the subject, That was published in 1867, the year following my attack on the so-called First Law of Motion. That treatment of the subject by Thomson and Tait I then regarded as the greatest difficulty I had to contend against, and it caused reiterated attacks on that law from different points of view to be a prominent feature in my publications. I am not now going to repeat arguments ; but ask you to consider as to whether Tait's treatment of the ' first law ' sixteen years afterwards, in the ' Encyclopædia Britannica,' published in 1883 (and alluded to in my letter of 6th May), is not a practical vindication of my attack on it, and the new position then assumed towards it illogical. Is it not evident that some recognition of my view regarding that law ought then to have been made, or, at least, an admission of the existence of a reasonable basis for my arguments ?

After I had expelled the so-called First Law of Motion from my philosophy I was for fully five years perplexed as to the true source of an important part of the mechanical action it was supposed to perform in the solar system. You can in your imagination make the radiation pressure of sunshine sustain the planets in their orbits against the action of the sun's gravitation ; but the imagination of the boldest of poets in the finest frenzy rolling could not, I think, make earthshine sustain the moon in its orbit against the earth's gravitation ; and we all, I think, admit that the sustaining force in the two instances must be of similar character.

The *push* of radiation pressure must thwart the free action of the so-called First Law in interplanetary space as certainly as the *pull* of astral gravitation ; but whilst the latter explains the vagaries of the gyroscope by the same action as that by which it sustains the moon in its orbit

above the earth, or the earth in its orbit above the sun, radiation pressure is baffled by even the single experiment shown by Professor Thomson on 7th April, 1906, and dealt with in my letters of 11th *idem* and some subsequent dates. I challenge you, Sir William Crookes, and all your etheritic colleagues to give any reasonable explanation of the phenomenon exhibited by Professor Thomson without the assistance of the simple action of astral gravitation which I have described.

Radiation pressure as certainly expels the 'First Law' as does astral gravitation, and they both require the transverse action of gravitation to sustain the continuity of motion supposed to be supplied by that law; but the acceptance of the theory of ether pressure could not directly lead to the discovery of that transverse action, which was a natural sequel to the discovery of the action of astral gravitation. The transverse motive force and the astral retarding force are both simple extensions of Newton's law of gravitation.

I am, dear Sir,

Yours truly,

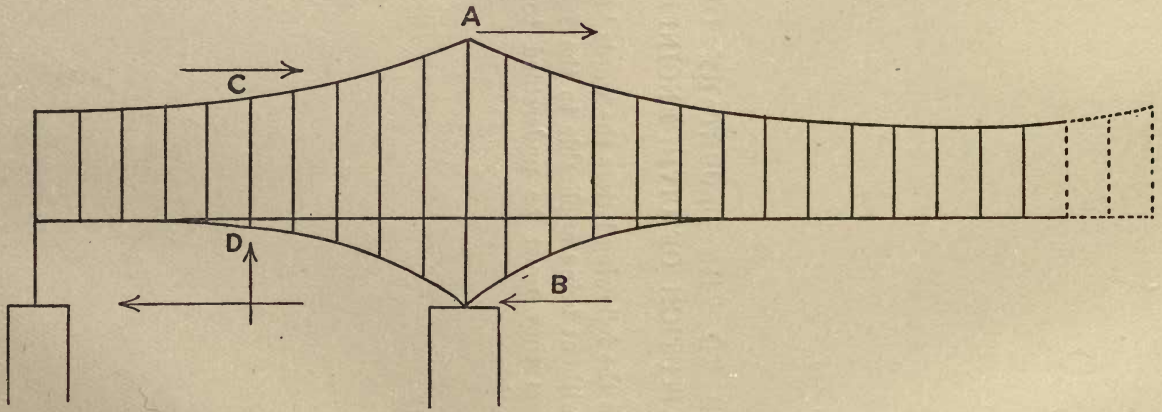
WM. LEIGHTON JORDAN.

LETTERS TO THE SECRETARY OF THE INSTITUTION OF CIVIL ENGINEERS

WHEREVER in the following letters the words 'this Institution' occur without being applicable to the Institution of Civil Engineers they must be understood to mean the Royal Institution.

SOUTH HALF OF THE QUEBEC BRIDGE

The dotted part had not been completed when the bridge collapsed in the afternoon of 29th August, 1907.



Bending Stress at A B on 29th August, with the foundation of the River Pier as a fulcrum, 8,500 tons.

Stresses at C D on 29th August, as described in connection with Diagram IV., 19,000 tons; the River Pier acting as a vertical fulcrum.

Letters to the Secretary of the Institution of Civil Engineers

IRON BRIDGES

15th April, 1908.

Dear Sir,—In designing the new Tyne Bridge, Dr. Harrison has adopted in principle the only method by which the vibration explained in my letter of 8th March, 1900, can be avoided; and his plan of five girders for carrying four lines of rails is one which I expect to see before long followed in the construction of all such bridges: four girders being given for three lines of rails, and three girders for a double line of rails. There is, however, in Dr. Harrison's design a survival of the old mischief which might easily have been avoided, and might perhaps (as well as I can judge from the small cross section which does not show detail) be remedied without too great an expense.

According to the cross section, the traffic stress is allowed in part to bear directly on the diagonal struts, and it must therefore create lateral vibration (with subsidiary vertical vibrations) of a character which would be avoided by making the traffic stress bear directly on the vertical support given by the girders, leaving the diagonal struts to do only their natural work of keeping the girders from getting skewed from their rectangular arrangement.

P.S.—12*th May*.—Let A B (fig. 1 in Diagram, p. 100) be the top part of a girder in cross section; C D a transverse girder on which the permanent way rests; and E a diagonal strut partly supporting the transverse girder.

Then the vibration above described (15th April) is compounded of the horizontal thrust, *v w*, and the vertical thrust, *x y*. And by clamping the cross girder on the main girder, as in fig. 2, the clamp would form an 'anchorage' against bending stress at D.

In the bridge under discussion the extent of the mischief caused by the stresses shown in fig. 1 is the creation of a vibration which must tend gradually to make its joints rickety. Those stresses may in that bridge be considered as

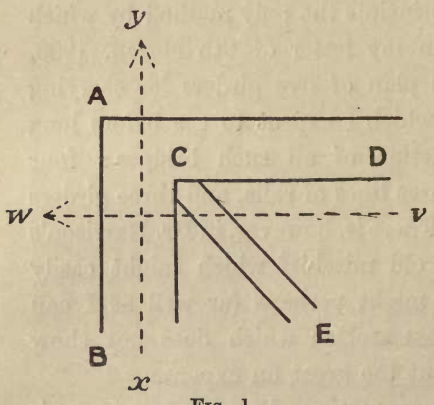


Fig. 1.

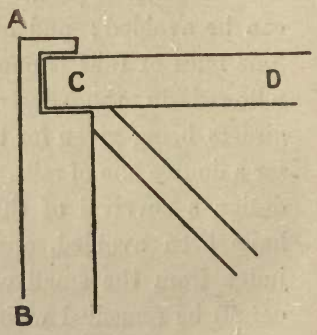


Fig. 2.

embryonic, though identical with what may under other conditions of length of leverage, both horizontal and vertical, become of such paramount importance as to cause instantaneous breakage of the struts on which they act, instead of merely a tendency to loosen their joints.

It seems to me that those stresses, which in the King Edward VII. Bridge may be termed embryonic, as they are not sufficient to do more than cause vibration, became developed in the Quebec Bridge at the moment of its fall, to a force of about 19,000 tons in the *anchor* arm, where not much more than 8,000 tons is the generally estimated

amount of stress (4,000 being tension along the top chord and 4,000 compression along the bottom chord). I have calculated the stresses in the *cantilever* arm at the moment of the fall on the same principle as in the *pro forma* calculation I gave in connection with Professor Fidler's recent paper on cantilever bridges; and (as shown in the table enclosed herewith) it gives 8,546 tons as bending stress on the main posts of the river pier at the time the bridge collapsed. As half of that is tension along the top chord, it agrees with the generally accepted uplift of about 4,000 tons on the anchorage.

The table shows that the stress which in the position of the main posts would, against one foot leverage of resistance be 2,692,400 tons, becomes against the actual leverage of 315 feet, 8,546 tons.

TABLE OF STRESS IN DIAGRAM, p. 98.

	Cantilever Weight in Tons.		Stress for 1 foot Vertical.
Main posts . . .	170 × 0		0
Panel.			
No. 1	1,280 × 28	feet =	35,840
„ 2	1,080 × 74	„ =	79,920
„ 3	920 × 140	„ =	128,800
„ 4	780 × 197	„ =	153,660
„ 5	670 × 253	„ =	169,510
„ 6	590 × 309	„ =	182,310
„ 7	530 × 365	„ =	193,450
„ 8	500 × 422	„ =	211,000
„ 9	470 × 478	„ =	224,660
„ 10	440 × 534	„ =	234,960
	7,430 tons		1,614,110

Girder Panels.			Stress for 1 foot Vertical.
No. 1	200 × 590	feet =	118,000
,, 2	200 × 647	,, =	129,400
,, 3	220 × 703	,, =	154,660
,, 4	70 × 759	,, =	53,130
Cantilever and girder .	8,120 tons		2,069,300
Part of large traveller .	600 × 500	feet =	300,000
Small traveller . .	200 × 730	,, =	146,000
Rolling stock and unplaced material . . .	100 × 703	,, =	70,300
And	200 × 534	,, =	106,800
Total weight . . .	9,220 tons		2,692,400
Stress on main posts of cantilever . .	$= \frac{2692400}{315}$	=	8,546 tons
Stress on the middle panel of the anchor span .	$= \frac{2692400}{140}$	=	19,230 tons

N.B.—The weight of the cantilever arm given above is taken from official statements; but its distribution is merely *pro forma*, to illustrate the argument.

If the feet of the main posts were firmly supported (during construction of the cantilever arm) against yielding to the compression thrust of 4,273 tons, then the corresponding tension stress along the top chord would be limited to the 4,273 tons all the way from the main posts to the anchorage. But instead of that being so, the stress which at the main pier was met by a resistance leverage of 315 feet was allowed in the centre of the anchorage arm to become concentrated against a leverage of only 140 feet, giving a stress of 19,230 tons, being 9,615 tons' tension along the top chord and 9,615 tons' compression along the bottom

chord; and the latter would to a great extent take the direction of uplift whilst the main posts of the river pier remained unbroken.

The stress in the centre of the anchor arm was thus more than twice as great as in any part of the cantilever arm, and would have continued to be so during construction until the two cantilevers met over the river and gave each other reciprocal support.

Extract from the Official Report of the Discussion on Cantilever Bridges on the 11th February, 1908

This can be followed only with the assistance of the diagram referred to as printed by the Institution of Civil Engineers.

RE PROFESSOR T. C. FIDLER'S PAPER ON CANTILEVER BRIDGES

Mr. Wm. Leighton Jordan said there were three points in connection with the cantilever paper on which he would like to make a few comments. Although he was not an engineer, it was a matter he had studied a great deal. He did not think the author had driven home two conclusions to which he had clearly pointed in his paper, and in no publication had he found those points so prominently brought to his notice as when reading Professor Fidler's paper. He mentioned with great trepidation, but with confidence, that Professor Fidler in his formula had taken what was not an appropriate quantity by dealing with l^2. In considering the stresses at any point in a cantilever, when dealing with a single unit it was necessary to take l^2 as the measure of the stresses to be dealt with, but directly an aggregate mass was dealt with l^2 was not a proper measure, as if it was used it was necessary to have a denominator

to correct it, and that denominator altered under different circumstances. In the paper where l^2 was dealt with the denominator was taken as 8 for the stress of $\frac{1}{2}\ w\ l$ at A_2B_2; but, for the stress of that same extension at C E, the figure 2 would be correct. If instead of l^2, in dealing with any aggregate mass one took $l \times \frac{1}{2}\ l$, i.e. what was called the sesquialteral, instead of the square, it came to less than the square and required no correction. He could not imagine that the formula given for the temporary stress at A_2B_2 had been used in actually building a cantilever, or any other bridge, and that an error must have got in there through some inadvertence. The dimensions in the diagrams seemed rather different from what they were in the book, but nevertheless he thought he could give the figures. For instance, taking the diagram of the cantilever ending at C E, and supposing it secure up to that point, the question was one of adding on, and in the paper it was dealt with as extended to z, which was a quarter of the independent girder. Taking that, as a simple quantity, as 20 feet extension—it might be anything—and taking it as 1 ton per foot length, the weight to be dealt with was 20 tons. That gave 400 tons for l^2, and it was quite impossible to get that out of the 20 tons as a stress at C E except the whole 20 tons were imagined to be carried out to the extreme end at which one was working, and then there was no material to support it; there was only an imaginary support remaining. The maximum to be obtained with the extension was what he had given, $w \times l \times \frac{1}{2}\ l$, which was the sesquialteral proportion, and that gave only a stress of 200 tons and required no denominator. The difference on applying Professor Fidler's formula to the stress at A_2B_2 seemed to be in the direction of safety in constructing the cantilever, but it was the business of the mathematician to give a correct formula and leave it to

the engineer to give the safety. If the formula was used in the opposite direction there would be an element of danger in it.[1]

There were two other points which he thought had been overlooked, or, at any rate, were not stated. In extending beyond the cantilever there could be no stress in the extension that could do any harm before the bridge gave way at B_2. The whole thing would come down at B_2, because the main stresses came in there. Following the diagram, beginning at A_0B_0, the ratio which the vertical bore to the arm extending from A_0 outwards to C was greater than it was further on. The vertical decreased in proportion to the horizontal arm until B_2 was reached, where the minimum was obtained, and passing from B_2 the maximum was reached again and went on increasing, i.e. the ratio between the vertical and the extension beyond the vertical, and therefore at the point of the minimum ratio of the vertical to the horizontal there was a weak spot, and that was the point of weakness at which it must come down.

A third point he wished to mention was that it did not seem quite clear that all the material was required whilst

[1] Let $l = 80$ feet
$A_2C = 30$,,
$A_2B_2 = 12$,,
$CE = 8$,,
and $w = 1$ ton per foot length 8 feet deep.
Then $\frac{1}{2}\,l + A_2C = 70$ feet; and $w \times l \times \frac{1}{2}\,l =$
$1 \times 70 \times 35 = 2{,}450$ tons
deduct 450 tons ($= 1 \times 30 \times 15$) for part of stress of A_2C
gives 2,000 tons stress against leverage of C E; or (for ratio of leverage A_2B_2 to CE as $1{\cdot}5 : 1$) $= \frac{4000}{3} = 1{,}333\frac{1}{3}$ tons for stress of $\frac{1}{2}\,wl$ at A_2B_2, representing its bending moment on that vertical if unsupported at FG.

Professor Fidler's formulæ for that bending moment are $\frac{1}{2}\,wl\,(X_2 + \frac{l}{4})$, which I make = 2,400 tons; and a second formula is given as $M_2 + \frac{wl^2}{8}$, which I make = 855 tons; the one giving a large margin of safety and the other a smaller margin of danger, unless I do not properly follow Professor Fidler's method.

in construction. Most of the material of the middle part was not required until the bridge was completed, and therefore the material could be passed out partially in narrow pieces. If in any cantilever B_2 was strong enough for the stresses that were to be brought upon it when in working order, it was quite strong enough to carry the middle part right across until the two met, not by building it all out perfectly, but by carrying it out in threads and meeting there.

SCOUR IN TIDAL RIVERS

Extract from the Official Report of the Discussion on the Netravati Bridge on the 24th March, 1908

Mr. Wm. Leighton Jordan remarked that an important point appeared to him to be clearly illustrated in the diagram of the Netravati Bridge, and that was with regard to the action of the scour. The thin black line showed the former position of the sand, and the blank space below it what had been scoured since the bridge was built. Of course much more had been scoured away than had been filled in above that line, simply because the river must make a deeper passage for itself in consequence of being narrowed. But on looking at the diagram it was very clear that there were two channels, one near the south side and the other near the north side, and between them the sand had been piled higher instead of being washed away. That, he contended, was absolutely necessary wherever there was a tidal channel of that sort. The tide as it came in under the bridge would not go out by the same channel; there might be very little difference, but a difference there must of necessity be, because as the water was moving the rotation of the earth must affect it, even if slightly. The diagram clearly indicated what for years past had been said not only by himself but by others as occurring, and he contended

that no engineer would ever manage a channel of that sort without clearly understanding that action. The tide would not go out by the same channel if it could possibly find another. It could be got closer together by narrowing that channel, and he believed quite successfully. . . . If it were possible for a tide to scour away on each side as it did in the open sea, it would gradually widen apart, make a broader bank, and sooner or later the channel would change. In the diagram it was prevented from changing, and that no doubt would be kept up as long as the bridge was kept standing.

29th May, 1908.

In the proof which I received from you on 14th instant and returned on the same day I notice that an explanatory statement based on what I said in the discussion of the sand banks of the Hooghly River, about three years ago, had been suppressed before sending me the proof. I now write to ask you kindly to consider the expediency of allowing my reference to the Hooghly River to appear in your Report of the discussion. And I further ask that, instead of omitting that reference, the argument I gave in the Hooghly River discussion may be given as a footnote attached to what I said on the Netravati River with a statement to the effect that it had been omitted from the report of the Hooghly River discussion through a misunderstanding.

The contribution to the Hooghly River discussion referred to in the foregoing letter is given in the following extract from the official report of that discussion, except that from the words 'James and Mary,' in line 7, to the words 'and when,' in line 13, was accidentally omitted in the office of the Institution.

Mr. Wm. Leighton Jordan said he wished to draw

attention to some facts connected with the shallows in the James and Mary Reach and in the Royapur and Moyapur Reaches, higher up. The momentum of the flood tide passing the sharp bend at Hooghly Point carried it past the descending stream, so that it commenced the ascent of the James and Mary on the right bank of the river; having, therefore, in relation to the down stream, an anti-cyclonic position, and there was confusion of currents, which caused a three-fathom shallow. Above Fulta Point the flood tide reached the left bank of the river and kept to that bank up to Brul Point, having, therefore, all along that reach a cyclonic position, and when the flood tide was on that side there was nothing less than five fathoms; though where the circulation was anti-cyclonic there were three fathoms. At Brul the flood tide crossed to the other side, and immediately above there was a shallow of three fathoms. Higher up, just before the flood tide went over to the right bank of the river, there were three fathoms again. He had pointed out, in connection with the Dublin Harbour, the manner in which any influx of water into the harbour, not brought in with the momentum in the direction of a cyclone, tended to create confusion in the harbour and to cause sediment, instead of running smoothly with the circulation. If brought in in the form of a cyclone, so as to take cyclonic circulation, it would run smoothly and get out. These reaches were an example of this. Where the flood tide was in a cyclonic course there were five fathoms; where it was not so, but was forced over to the opposite side of the river, there were shallows. He considered that wherever a tide branched off from the parent tide there was a normal tendency to cyclonic circulation. He believed the main force of the rising tide came up the Hooghly estuary to Sangor Island by the eastern channel, giving an indication of a cyclone in that part. In the Bay of Bengal

itself the tide was an anti-cyclone; it was a tide directly raised by the sun and moon; but in such an offset from it as the river the tide tended to take the form of a cyclone. It had nothing to do with the gravitation of the sun or moon, but was dependent on the earth's rotation. As long as it could keep its cyclonic circulation it ran smoothly, but when once that was broken by any cause whatever there was confusion and sediment. The action of the earth's rotation on the water in the river would never be correctly understood until it was recognised that, wherever the moon raised a tide by its direct action, that tide was an anti-cyclone, exactly as in the Bay of Bengal. In the Southern Indian Ocean there was another anti-cyclone, and also another in the South Pacific and in the South Atlantic. The southern edge of those anti-cyclones was in the great Southern Ocean. They ran one into the other just round that part of the world where it was generally stated in all text-books on the tides that the tide ran from east to west, and the derivative tides came from that. That view was first put forward by Lord Bacon, and his authority had dominated everybody, and there had been a general disinclination to dispute it. It had been disputed from time to time, but little attention had been given to it. Until it was recognised it would never be possible properly to connect the action of the earth with river motion. As a matter of fact, the tides came from the Pacific to the Atlantic round the Horn from west to east. At the Cape of Good Hope, round the south of Africa, the tides came from west to east, and all round the south of Australia the tides were also west to east. Where the tide was supposed to run from east to west, it really ran in the opposite direction, and no such tide existed as that which was supposed to be the predominant tide in the world, of which all the others were offsets.

16th April, 1906.

As a sequence to my letter of 10th instant, which was called for by the letter received from you that day, it has clearly become my duty to confirm my letter of 2nd December last and those preceding letters on which it was based.

The position is that—

1. On the 17th January, 1905, Mr. Vernon-Harcourt read a paper in which he specified the most important of the sand-banks which obstruct the navigation of the Hooghly River, and proposed works for guiding the tidal action in the river for the purpose of their removal.

2. I gave in the discussion an explanation applicable to each one of the specified sand-banks, showing how they are created and maintained by the tidal action.

3. Your 'Publication Committee' could not possibly have had my argument properly before them when they considered it not to be applicable to the subject of the paper.

4. Suppose Professor Vernon-Harcourt had been so chivalrous as to insist on replying to my argument; you could not then have suppressed it. Professor Vernon-Harcourt and I have for more than thirty years held rival views regarding tidal action, and his acceptance of the protection of your 'Publication Committee' to enable him to escape the responsibility of replying to my argument, spoken in his presence, is a course on which I will not comment.

The character of the Institution, as regards honesty of purpose in its discussions, combined with precedent study of the subject discussed more readily gaining consideration than personal favouritisms, is in the hands of the Council rather than in those of individual members. . . .

5. Sir Guilford Molesworth was President of the Institution at the time of the discussion, and himself took part

in it. It is mere matter of fact that my remarks were directed to the main point of Professor Vernon-Harcourt's paper; whereas it is no disparagement of Sir Guilford Molesworth's very interesting remarks to say that they did not grapple with the difficulties, the removal of which was Professor Vernon-Harcourt's main object.

In spite of that Sir Guilford Molesworth acquiesces in the action of your 'Publication Committee' in reporting his remarks whilst persisting in the suppression of my argument on the false pretence of its not being applicable to the subject of the paper.

* * * * * *

It is a comparatively small matter for erroneous opinion sometimes to find place in our published Proceedings. Diametrically opposite views are given in discussions where one must of necessity be absolutely wrong; but the essential point for the maintenance of the character of the Institution is honesty of purpose in discussion and the maintenance of fair play by the Council between rival arguments which are known to be results of precedent study of any subject formally allowed by the Council to be brought forward for discussion.

4th June, 1908.

I think it would be quite out of place for me to refrain from pointing out, in reply to your letter of yesterday's date, that the 'misunderstanding' referred to in my letter of 29th ultimo is recorded in your letter of 4th July, 1905, where you tell me that our 'Publication Committee' had decided that what I said when I spoke in the Hooghly River discussion was not applicable to the subject-matter of the paper.

It became clear at the time that the Committee were misled through not having a correct report of what I said

before them; because, in fact, the main point of the paper was directed to the obstructions caused by sand-banks in the river, and I gave an explanation of the manner in which the conflicting action of the tides and the river current tends to cause such banks, and the proper relative positions to give (where possible) to the tidal and the river current in order to keep a deep channel open—that being a subject to which I had given special consideration from time to time during the previous fifty years.[1]

The Committee are entitled to consider my cyclonic action of the currents wrong, and Mr. Ellis's straight course action correct; but to say that one and not the other is applicable to the subject-matter of the paper is simply at variance with the clear facts of the case.

I therefore again urge the expediency of the course suggested in my letter of 29th ultimo being followed.

8th June, 1908.

Your letter of 4th July, 1905, came upon me like a bombshell, as I had not for a moment imagined such an unceremonious exclusion of an argument offered in discussion to be possible in our Institution; and as my letter of 4th instant replying to yours of the previous day stands unacknowledged, I think it expedient to supplement that letter with a further remonstrance against the course which your letter of 3rd instant shows to be contemplated rather than wait until the exclusion of what I said in the Netravati discussion becomes an accomplished fact.

If the principle involved in the idea that knowledge gained in the study of sand-banks in the Hooghly River is not to be referred to in discussing the sand-banks of the

[1] Extracts from my contributions to the 'Buenos Ayres Standard' are given on pages 121 to 124.

Netravati had always been acted on, a large proportion of the most useful discussions in our Proceedings would have been excluded; and in the present case there is a special reason for allowing the action of the currents in the Hooghly to be referred to in discussing those of the Netravati, inasmuch as what I said in the Hooghly discussion was wrongly omitted, and the Netravati discussion chances to offer a suitable opportunity to remedy that mistake. The Hooghly and the Netravati are both tidal rivers in the positions under discussion, and the phenomena in each appropriately serve for illustrating those in the other.

You properly inserted in the Hooghly discussion Mr. Ellis's argument as to the momentum of the river current tending to make it persist in a straight course; but I protest against the exclusion of my argument to the effect that the earth's rotation changes that straight course to a curve in relation to the surface of the earth over which the water moves; and that where a rising tide meets a river current the streams can run smoothly only when able to take such relative positions as to form complementary parts of a cyclone.

That argument would have been imperfect in the Hooghly discussion without showing its connection with tidal action in the open sea; where, as I stated in that discussion, the distribution of cyclones and anti-cyclones is such that all round the temperate zone of the Great Southern Ocean the tide waves flow from west to east, thus constantly *meeting* instead of *following* the moon all round the world in that latitude.

The Proceedings of our scientific societies and the pages of our leading scientific journals have in recent years teemed with elaborate mathematical demonstrations of the manner in which tidal friction westwards must retard the earth's rotation; which is all very splendid as a display of

mathematical skill, but is all outside the region of practical science, as in fact the tide gives just as much friction eastwards as westwards.

As you tell me in your letter of 3rd instant that you are guided by the Institution's interest as to publishing remarks offered, I ask you to consider as to whether it may not redound more to the credit of the Institution to take the lead in allowing fair discussion to be commanded for the theory I have brought to bear on the sand-banks of the Hooghly and the Netravati, rather than to persist in holding the position of an item among the scientific societies in which a display of mathematical skill is more highly appreciated than commonsense unravelling of the action of the forces by which natural phenomena are practically controlled.

The gradual growth of the Admiralty tide tables has swept away the waves which Bacon and Whewell supposed to flow from east to west round the Great Southern Ocean; and I, as one who cares as much for the 'general interest of the Institution' as any man among them, ask our Council to take the lead in recognising that practical advance in our knowledge of the world.

A correct knowledge of the scouring action of currents in tidal rivers is of importance to engineers who have to deal with such localities.

17th June, 1908.

Your letter of yesterday's date makes it clear that a persistence in the exclusion of my arguments regarding the Hooghly River is contemplated, though apparently not yet resolved; and I therefore endeavour to reinforce my letters of 4th and 8th instant in protest against that course.

As you now tell me that there is no question of my desire to promote the interests of the Institution, I would not say it is unfortunate that you allowed a pretension to the

contrary to appear to be made, if it were not that you now tell me it is not possible for everything contributed to the Institution to be published, thereby ignoring the special reasons I have given showing that the highest interests of the Institution demand a rectification of the official report of the Hooghly discussion.

I urge upon you that in the actual discussion of the Hooghly sand-banks I explained the curving of the river stream and tidal current in opposite directions under the action of the earth's rotation, and also the circumstances under which that conflicting action tends to build up sand-banks. But, instead of that argument appearing in the official report, there is an elaborate argument based on the idea of the momentum of the currents acting in straight lines without the slightest recognition of those straight lines being affected in relation to the surface of the earth by the movement of that surface.

The official report of the discussion is therefore an absolute misrepresentation of the views held by our Institution as represented in the discussion; and as the Netravati discussion offers a suitable opportunity for rectifying the omission of my argument in the Hooghly discussion, I urge the Council to allow it to appear in the form suggested in my letter of 29th May.

You tell me in your letter of the 3rd instant that as regards publishing remarks offered in discussion the Council are guided by the question of the advantage and general desirability in the Institution's interest. That certainly is what ought to guide them; but they are just as certainly mistaken in supposing it to be to the Institution's interest to suppress views originated by any associate or member who may have given more consideration to a subject brought forward for discussion than any man among them.

What the interest of the Institution calls for is a faithful

report of what may be said by the Institution's own experts on any subject submitted for discussion. As regards the question raised in the Hooghly discussion I was the leading expert; and it has become clear to me that some practical engineers are now guided by my views, spoken in that discussion, notwithstanding the Council's refusal to allow them to appear openly in the Proceedings of the Institution. Will that course redound to the highest interests of the Institution?

Secondly, there are several other cases in which the treatment of contributions I have offered on subjects under discussion has been as strange as in the foregoing.

You have now under consideration my contribution bearing on the Quebec Bridge disaster, in connection with which I beg to recall attention to the fact that on 8th March, 1900, I sent you an explanation of the loosening of rivets in the underside of iron girders, which a member of the Council had at a previous discussion declared to stand unexplained. No notice was taken in the Proceedings of my contribution, but the form of girder then in use was soon afterwards modified in various manners, all more or less partially escaping from the mischievous action I had indicated; the last important girder bridge (the King Edward VII. Bridge over the Tyne) being the best yet described at the Institution meetings, though even that leaves great room for improvement, as I have pointed out in the correspondence discussion on it. I have in that contribution pointed out that the stress overlooked in the construction of the King Edward VII. Bridge was also overlooked in the construction of the Quebec Bridge; but in the latter it had accumulated at the moment of the disaster to such an extent as to create a stress of about 19,000 tons where less than 9,000 was supposed to exist.

I now urge that if my contribution to the subject in the year 1900 had been admitted into the official report of our

Proceedings, the attention drawn to the point I had raised, instead of only influencing a few who vaguely heard of it without fully understanding it, would have been properly considered in the engineering profession and would most certainly have prevented any such disaster as that of the Quebec Bridge.

That it may not be thought that I hastily at random intervened in the matter of bridge construction, it seems expedient for me to say that twenty-five years ago I looked forward to being able not only to select the spot for crossing the Uruguay River for connecting Rio de Janeiro with the River Plate by rail, but also to design the general features of the bridge; so the subject was not new to me when I intervened in the year 1900 in the discussion in our Institution.

The cruel death of each of the men who lost their lives in the fall of the Quebec Bridge is a consequence of the want of judgment shown by our Council in suppressing the explanations given in my letter of 8th March, 1900.

1st July, 1908.

I am quite in the dark as to whether my letter of 17th June is having consideration; but think it well further to urge that not merely the most ordinary requirements of fair play to me personally, but also the true interests of the Institution make it expedient for the Council to allow my argument regarding the sand-banks of the Hooghly to be reported in our Proceedings, as suggested in my letter of 29th May last.

In connection with the above question I wish now to recall your attention to the discussion of the paper on the steam turbine, read on 5th December, 1905.

In that discussion Mr. Henry Davey asked for an explanation of the backward movement of a loose bush which surrounded the spindle of the turbine. That backward

motion of the bush *against* the direct action of the force by which it is set in motion is a phenomenon of the same character as the eastward motion of the tide waves of the temperate zones *against* the course of the moon, by whose force of gravitation the tide is raised. The explanation asked for by Mr. Davey was familiar to me in connection with other phenomena; and as I did not hear it given in the discussion, I sent it to you on 16th December, 1905, with some further details on 26th *idem*. The two explanations I heard at the meeting appeared to me to be at variance with each other, and neither of them conscious of the mathematical evidence given by the figures which I sent to you. Your report of Mr. Parsons's reply shows him to have correctly understood the phenomenon, though I did not myself gather that as I listened to what he said in reply. If I had done so, I would not have written to you on 16th December, 1905.

I think it, however, well now (in face of other omissions of my contributions) to say I do not believe that the clear and explicit explanation I sent to you in writing on the 16th December, 1905,[1] and *a fortiori*, the detail I sent to you on 26th December, 1905,[1] ever existed in writing before those dates; and therefore I think it would have been creditable to the Proceedings of the Institution to be the first to give a printed statement of the mathematical argument as I sent it to you.

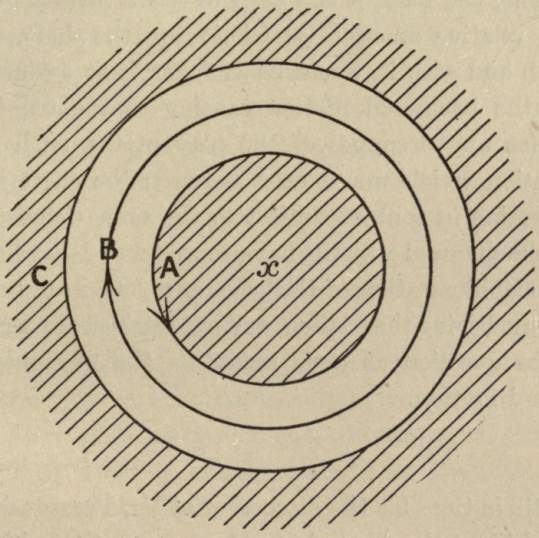
Other phenomena of the same character are explained in my published works, but not, as far as I know, in any other published works. The Council can give public credit to colleagues who give them second-hand knowledge, first given in my works, but it would redound more to the credit of the Institution in the long run to give credit to the first discoverer of the general principles by which phenomena of that character are controlled.

[1] Now appended to this letter on pp. 119 and 120.

16th December, 1905.

In Tuesday evening's discussion opposing explanations were given regarding the motion of a loose bush, B, placed between a rotating shaft, x A, and a fixed case, C, illustrated in the diagram below.

It was stated that, as a matter of fact, with the shaft



A, x. The turbine shaft.
B. Loose cylinder, closely fitting between the turbine shaft and the fixed cylinder, C.

rotating 5,000 times a minute, the bush revolved thirty times a minute in the opposite direction.

I say that as the bush cannot completely escape the friction caused by the rotating shaft, and as it is free to revolve in either direction round the shaft, it takes that direction and velocity which enable it to escape as much as possible of the friction. I have elsewhere given figures which show how the backward motion of the bush reduces the friction to which it would be subjected if it revolved with the rotating shaft.

The point is that, supposing the bush to give an increase of 1 per cent. to the radius, then, if it revolved completely with the shaft and its friction be represented by $5{,}000 \times 101^2$ $(= 51{,}005{,}000)$; then the substitution of thirty revolutions backwards changes the relative amount of friction on the bush to $(5030 \times 100^2) + (30 \times 101^2) = 50{,}606{,}030$; which constitutes a reduction of $\frac{3}{4}$ per cent. ; in addition to which, I presume, the play of the bush causes a further decrease.

The relative amounts of friction on the shaft, without the bush and with it, would be as $5{,}000^2$ is to $5{,}030^2$ respectively, the backward motion causing that extra friction, unless (as above supposed) the play of the bush reduces the friction which would exist between the shaft and the rigid bearing in contact subject to the same clearance.

In the diagram which I have reproduced I have omitted any special illustration of the ' clearance,' as, in my opinion, merely modifying the friction, and not intrinsically connected with the question at issue regarding the phenomenon of backward motion.

26th December, 1905.

If still in time for the discussion of Mr. Pearsons's paper, I should like to be allowed to add, in connection with what I wrote on 16th instant, an explanation of the reason why I presume that the play of the bush there referred to tends to reduce friction to a greater extent than the amount there specified.

I doubt that anyone now seriously attempts to question the fact that friction and gravitation are identical forces; the former term being applied to the action of gravitation between bodies in contact or apparent contact.

Let the difference between apparent contact and absolute contact be any amount, say 1, making that figure represent the minimum distance between the shaft, x A, and the bush B (diagram of 16th December) ; and let the mean distance

be 2. Then a minimum of 1 at any point creates a maximum of 3 at the opposite point of the shaft. And the retarding action (whether termed gravitation or friction), being inversely as the square of the distance, is, at the minimum and maximum distances, as 9 is to 1 respectively; giving a mean resistance of 5. To get the position of minimum friction the bush therefore tends to keep itself equidistant from the shaft at all points, as the resistance at each point is then represented by 4 instead of the above mean of 5.

The resistance is as the square of velocity under similar conditions; but the velocity which is sufficient to keep the bush firmly at its mean distance all round changes the condition to one of lower normal resistance; because a lower coefficient of friction is created, which more than counterbalances the normal increase at the rate of the square of the velocity. That reduction of the coefficient of friction with increase of the shaft's velocity of rotation may be indefinitely extended by molecular action on the same principle as that described in connection with the bush. Even when no bush is purposely inserted, centrifugal action must tend to create one and to leave a vacuum between it and the shaft.

THE EFFECT OF THE EARTH'S ROTATION ON THE RIVER PARANÁ

Extracts from the Buenos Ayres 'Standard' of 20th and 30th May, 1871. *See letter of 4th June*, 1908, *page* 111

20th May, 1871.—The Paraná from its junction with the Paraguay, where it chafes against the rocks of Corrientes and refrains from bearing against and washing away the soft soil which forms the low ground on its right bank, shows a general tendency to bear against the comparatively high and firm ground on its left bank. The rocky shore on

the left bank of the Plate also prevents it from swaying away on that side, and it shows no tendency to wear away the comparatively soft soil on its right bank.

This apparent tendency of these rivers to bear against their left banks is corroborated by the streams of the delta of the Paraná. The Carapachay, once an open stream, is now choked up with weeds. The stream of the Antiquera is too sluggish to prevent the growth of weeds in it, and it seems likely to be before long in the same state as the Carapachay. The Capitan is gradually working a way for itself through the Tres Bocas, and if not kept to its present course by artificial means, will cease to run through the Abra Nueva. These all indicate a tendency of the river to bear away against its left bank.

Two causes may be contributing to this result—the one a gradual upheaval of the Andes, together with the land stretching westwards to the river; the other an effort of the river to follow the natural curve which, as it runs from the direction of the equator, the rotation of the earth tends to give it. . . . The tendency of the river shows that works undertaken with the object of throwing the river over towards its left bank, so as to make the stream deep and compact on that side, would be assisting it in its natural course.

If then an attempt be made to make the straggling streams of the Paraná and Uruguay run in one deep compact stream to the ocean, it should be commenced by turning all the right bank streams of the delta of the former over towards the Guazu. This might be accomplished by running a railway viaduct like that of the Boca Railway from above the Tigre to the Guazu, and then gradually closing the spaces between the piles without endeavouring too suddenly to force the stream to change its course. Any breakwaters constructed for improving the harbour of Ensenada would

have a favourable tendency, and might afterwards be extended for the express purpose of improving the main channel of the river. The harbour of Ensenada can be more immediately utilised than any other point on the river. . . .

30*th May*, 1871.—Delta says that many points of the Paraná confute the assertion that it is thrown against the comparatively high land of its left bank by the upheaval of the land on its right bank, and by the earth's rotation. I am aware that in some places the river is at present abrading its right bank; but as long as the general tendency is to follow the course of the high land on its left bank, this occasional abrasion of the right bank is just what might be expected to result from the upheaval suggested. For instance, suppose in a part of the river where the east shore is rock the land be tilted upwards from the west, then the river, not being able to fall away eastwards, must cut away the uplifted soil of its west bank until it makes room for itself at its former level.

Irregularities in the upheaval might also in places cause it to sway at times against its west bank without any insurmountable obstruction appearing immediately opposite. The abrasion now going on between Rosario and San Lorenzo appears to me, judging from the inclination and general appearance of the ravines cut by the rain-water as it runs off the land, to have commenced somewhat recently. Its immediate cause may perhaps be traced to the accidental formation of some obstruction in the river; but I think it more likely that it results from an irregularity in the upheaval of the land. If the requisite information can be obtained, it may perhaps be found that the date of the commencement of this abrasion immediately follows the occurrence of an earthquake in Chili. In the estuary of the Plate the almost complete absence of abrasion on the

right bank seems to indicate that, even before it reaches so high a latitude, the rotation of the earth tends to carry the river due eastwards, in the same manner as in the temperate zones it tends to carry the water of the ocean eastwards. . . . The Guazu is the natural channel through which the river endeavours to cut its way; and surplus water pours through other channels only where compelled to take them in order more readily to find its level. If the natural channel be obstructed the river will be constantly endeavouring to regain it by undermining the obstruction; whereas in other channels obstructions once formed tend to become more and more consolidated.

PRESSURE AND STRESS IN MASONRY DAMS

30th January, 1908.

I have just re-read your letter of 23rd instant, and address you for the purpose of pointing out that in Tuesday evening's discussion no speaker rose to address the meeting without being first called on by name by the President, and I have decided not to make myself exceptional in that respect. If it were the custom for members to rise without being called on by the chair, as in the House of Commons, there would be nothing invidious in doing so, but it seems to me most unfair for the Council to impose such a course specially on me.

I therefore now submit in writing the following new view of the distribution of stress created by water pressure in masonry dams, and also the correction of an important error on that point in the paper presented by Sir John Otley and Dr. Brightmore.

In that paper it is argued that if the width of any dam increases as the depth, then the shearing stress on a horizontal plane must decrease as the height of the plane increases.

That view is arrived at by an error as to the amounts of water pressure at the surface and bottom of the dam, which will, I think, be quickly recognised by the authors if they reconsider the argument they have given on page 8 of their paper.

Eliminating that error, the water pressure is made to vary equably with the width of the dam in question, instead of relatively increasing with increase of depth.

Having cleared that mistake out of the way, I will now point out that what the dam-builder has to face is *the stress created by the pressure*, which stress is in fact precisely the same in quantity, though not in direction, at all depths; and 'the mean intensity of the shearing stress on a horizontal plane,' dealt with in the paper, must therefore (in that dam) *increase* instead of decreasing as the height of the plane above the base increases.

The fact is that the water pressure increases as the depth; but the surface water acts against the side of the dam with a leverage increased in proportion to the depth; and therefore any increase of the depth of the water increases the thrust, or stress, *at the surface and at all depths*, exactly in proportion as the depth of the dam is increased.[1] Therefore, in a dam retaining the same depth of water all along it, the stress created by the water pressure is the same in amount at all points; but on the inner face there is a partial change from shearing to tension stress as the height from the base increases; and on the outer face there are increases of compression and shearing stresses corresponding as to quantity with the tension stresses on the inner face.

Sir Benjamin Baker with the jelly on the 28th March, 1905, reminded me of Christopher Columbus with the egg; and, immediately after the meeting of that date, I wrote to ask him if he was aware that the stresses he had described

[1] Modified in next letter.

were identical with those by which any pebble is rolled along the bottom of a stream.

In face of the stresses shown by Sir Benjamin Baker's jelly and confirmed by the two independent sets of practical experiments now before the Institution, it is clear that any pebble rolled along by a stream of water gives the clue to the stresses created by water pressure in a masonry dam.

8th and 12th March, 1908.

Table of water pressure and leverage stress against 1 *foot width of the vertical height of masonry dams in each* $\frac{1}{10}$ *part of their depth.*

With unit of depth =1 foot the unit of pressure is $31\frac{1}{4}$ lb. ; and for higher dams the unit of pressure increases as the square of the unit of depth.

Depth.	Pressure.	Stress.
1	1	1 × 19 = 19
2	3	3 × 17 = 51
3	5	5 × 15 = 75
4	7	7 × 13 = 91
5	9	9 × 11 = 99
6	11	11 × 9 = 99
7	13	13 × 7 = 91
8	15	15 × 5 = 75
9	17	17 × 3 = 51
10	19	19 × 1 = 19
Total units . 10	100	670

Can you kindly arrange for me to substitute the above for a paragraph in my contribution to the correspondence on masonry dams ?

Addition to table (dated 8th March, 1908) of water pressure and leverage stress in masonry dams.

20th May, 1908.

For the purpose of giving an illustration of the method of using the above table I apply it to the Assuan dam as it is at present, and also as it will be when the proposed increase of its height has been effected, taking, as a basis for the two estimates, Sir William Willcock's recent statement that what is proposed is to change the head of water of 22 metres to 28 metres.

That constitutes a change from a present height of about 72 feet to about 92 feet.

For the present dam the pressure :—

100 in the table, $\times 7{\cdot}2^2 \times 31\frac{1}{4} = 162{,}000$ lb.

And the stress :—

670 in the table, $\times \frac{2}{3}$ of $7{\cdot}2^3 \times 31\frac{1}{4} = 5{,}209{,}920$ lb.

For the proposed dam the pressure :—

100 in the table, $\times 9{\cdot}2^2 \times 31\frac{1}{4} = 264{,}500$ lb.

And the stress :—

670 in the table, $\times \frac{2}{3}$ of $9{\cdot}2^3 \times 31\frac{1}{4} = 10{,}869{,}186$ lb.

The pressure being as the square of the depth, and the stress involving the multiplication of the pressure by the depth, the latter would be as the cube of the depth but for the reversed direction in which the figures have to be applied as increasing multipliers.

A point in this method which seems to me still an open question is as to the ratio of the stress unit to the pressure unit which I have taken as a ratio of equality, because it certainly cannot be *less* and I do not see how it can be more.

These estimates of stress are in excess of what has been generally supposed ; but in venturing to submit them to the Institution of Civil Engineers I am encouraged by Sir Benjamin Baker's statement at the meeting of 28th March, 1905, to the effect that many mathematicians had been

working at the problem of stresses in masonry dams during the previous twelve years, and in that time perhaps half a dozen had been washed away; but he could not mark any great advance in the theory.

FLYING

18th April, 1907.

Sir Benjamin Baker, presiding at the meeting of the Society of Arts last evening, said that Institution is the only one open to all subjects of general interest.

I do not dispute that statement; but it is of no use addressing it to me. The difficulty with my views has become personal, and my reasons for appealing specially to the Council of our Institution I have clearly stated.

In reference to the action of some small screw-propeller flyers which Major Baden-Powell declared to be 'inexplicable,' Sir Benjamin said Lord Rayleigh considered his mathematics unable to grapple with the question.

In that Lord Rayleigh is completely mistaken: it is not his mathematics that are at fault: the fault is that he does not know how to present the question to his mathematics for solution.

If a carpenter presents his material to the wrong end of his chisel, or to one side of it, he gives the chisel no chance.

Major Baden-Powell's difficulty was that his flyer, instead of progressing in the direction the screw endeavoured to drive it, either plunged head first downwards or, if prevented from doing that, then would not advance at all, but turned over backwards and fell in that manner.

The mechanical action by which *gravitation* turns those flyers over, as shown in Major Baden-Powell's experiments,

is clearly and explicitly given in the Report of a lecture which I delivered in Willis's Rooms on the 3rd of November, 1877, which you have in our library; and if Lord Rayleigh would take the trouble to understand the question he would find his chisel quite competent to deal with it.

The result would be either to give an angle at which the screw would be adjusted to the action of gravitation, or else to show it to be impossible for those forces to run together harnessed to Major Baden-Powell's flyers.

In the latter case the inference would be that the difference between those flyers and a bird is that the latter is able by a natural effort to create a vacuum in some part of its body, so as to neutralise the action of gravity. The position of the gizzard and the immense strength of its sinews seem to make it well adapted for such a purpose.

I have said enough in some of my previous letters to show that it would be out of place for me to offer any apology for addressing you on this subject.

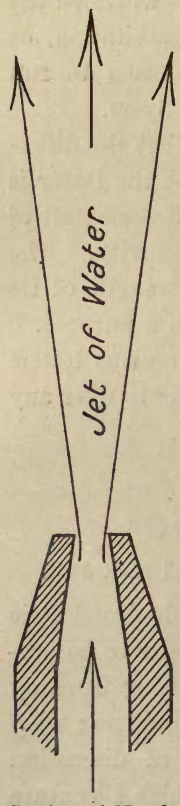
THE SPREADING OF THE JET OF WATER

29th April, 1907.

After hearing Mr. Kellow's paper on hydro-electric power, I obtained a printed copy of it, in order to be prepared to make some remarks on the following Tuesday in case of finding a chance in the rush of speakers; who, however, maintained an interesting flow of discussion without touching the important point on which I venture to comment.

Fig. 5, on page 10 of Mr. Kellow's paper, shows a jet of water issuing from a nozzle; and the author treats the dispersion of the jet, as it flows away from the nozzle, as being due to the difference of velocities at its centre and periphery. That idea clashes with the true basis of all hydraulic power.

The fact is that, as water is incompressible, and as its velocity must of necessity decrease as it gets away from the projectile force of pressure inside the nozzle, it must spread so as to give a sectional area increasing as the velocity decreases. The advantage of utilising the force close to the nozzle, instead of at a distance from it, is due solely to the fact of hydraulic power being as the square of the velocity, so that with the decreasing velocity beyond the nozzle the power rapidly decreases.



Section of Nozzle.

The author records an interesting experiment in which, by arranging an obstruction in the centre of the nozzle, he reduced the dispersive action. That, however, merely shows that the obstruction must have created an eddy just outside the nozzle; and the decrease of velocity would be just the same at any given distance relatively as in the case of the unobstructed jet, though the eddy neutralises part of the area at the nozzle. The dispersive action is prevented only by wasting power at the nozzle.

I ask the Council to consider whether it would not have been more creditable for our Institution to have avoided printing such an error in fundamental principles of the engineering profession. I do not mean that the paper ought to have been rejected; but that, in view of its merits and importance in other respects, the 'Publication Committee' might have suggested to the author a reconsideration of the point I have specified. Any such friendly hint

would doubtless have sufficed to lead him to eliminate the error.

* * * * * *

An important subject, on which I have on several occasions addressed you, has been forced again upon my attention by Mr. Savile's paper on the new railway bridge at Bristol, and Mr. Allan's on the Pyrmont bridge, which were read and discussed at the last two Tuesday meetings. It is rendered all the more appropriate for me to comment on the subject inasmuch as it serves to illustrate the 'personal' element in opposition to new views, to which I have alluded in the letter I addressed to you on 18th instant.

In a paper on iron bridges, read about two years ago, Mr. Marriott describes (p. 7) how some old girders were removed from a bridge and replaced by others of a different construction. It is mere matter of fact that the new form of girder reduces by about one-half the worst of the leverage stresses on the rivets which I had described in a letter which I addressed to you on the 8th March, 1900.

The first step towards getting rid of mischievous stresses on rivets is a knowledge of the mechanical action by which they are caused, and I do not think that action had been clearly defined in anticipation of my above-mentioned letter. At any rate, neither the discussions I had listened to in our Institution, nor any works to which I obtained access, explained them; and (writing from memory) more than one work alludes to the loosening and breaking of rivets in the under side of girders as inexplicable. That was the position when I addressed you on the subject. Of course the new form may have been arrived at quite independently of me; but it was clear that at the time I wrote my letter of 8th March, 1900, the speakers I had heard in discussions and the writers I had read did not

then understand those stresses ; and the medal, or premium, given by the Council five years afterwards in recognition of the new form of girder forms a thorough justification of my action in addressing the Council on the subject. This, which is really a very small point in the work I have done in the course of the past forty years, is pulled into light again by the papers on the new Bristol bridge and the Pyrmont bridge. The sections published with the latter paper do not show precisely how the leverage stresses I explained have been guarded against. But they show that the method is not that adopted by Mr. Marriott in the work for which he obtained the Council's medal. As far as I can judge from the published sections, the vertical riveting which resulted in the leverage stresses which I explained has been abandoned altogether and substituted by horizontal riveting, which almost completely annuls the leverage and makes the work depend on shearing stress, which seems pretty certain to be much more effective in withstanding wear and tear. And in the Bristol bridge both methods of avoiding the mischievous stresses appear to have been availed of, as well as I can judge from the paper. At any rate, no vertical riveting appears in the position of that which I criticised seven years ago.

A remarkable phenomenon in connection with this question is that, as far as I have been able to ascertain, the reason for these new forms of girders being superior to those now obsolete is not anywhere explained in the published Proceedings of our Institution. It seems to exist only in my above-mentioned letter, and, indirectly, in the explanation of the same action in my unpublished paper of 21st December, 1900.[1]

[1] The error as regards hydraulic action which the foregoing letter indicates has been published in the Proceedings of the Institution, as if it had not been perceived by anyone present at the discussion !

RESISTANCE TO THE MOTION OF SHIPS

22nd March, 1905.

I understood at the discussion yesterday week that the purpose of Lord Brassey's paper was considered to be to discuss the merits of different sizes of ships, and not their shapes. As, however, Sir Wm. White and Professor Biles both alluded yesterday to United States battleships as drawing generally about two feet less than ours of the same displacement, and seemed to consider it indisputable that the lesser draught places them at a disadvantage, and is forced on them, nevertheless, in consequence of the shallowness of their home ports, I hope I may be allowed to add a short contribution to the discussion for the purpose of suggesting that the lesser draught may prove to be really advantageous.

According to the principles originated by the late Mr. Wm. Froude, based on the idea that the inertia of the water does not resist the ship's way, the deepest draught consistent with stability ought to be the most advantageous. But I beg to be allowed to urge that the above-mentioned idea is erroneous and has probably given our battleships the same inferiority to those of the United States, as was proved in the case of our yacht 'Shamrock III.' as compared with the American yacht 'Reliance,' in the recent Cup race.

Suppose, for the sake of illustration, one of two ships of the same displacement to have a mean draught of 11 feet and the other 10 feet. Then, if the water displaced had all to be lifted to make way for the ships going at the same speed, one would have to lift its displacement 11 feet in the same time as the other would have to lift it only 10 feet, and the forces requisite to do the work would be as the squares of those relative velocities, or, : 122 : : 100. That is to say, the ship with the deeper draught would, for that

special purpose, have to expend one-fifth more power than the other.

There can, however, be no doubt, I think, that ocular evidence of the movement of water near a steamer in calm weather and the slip of the screw, show that some quantity of water must reach the stern of any steamer by passing under the ship's bottom with greater velocity than the ship progresses through the ocean. The increase of friction to be overcome in consequence of that action cannot be equivalent to the work saved in lifting the water, as the water will take the easiest route out of the ship's way. But whatever water may sink right ahead of the ship to pass under her, instead of being lifted, must reduce the effective action of the screw by increasing the slip.

The part of the water which is not lifted may therefore be more detrimental to the ship's progress than any equal quantity which is lifted.

I hope this note of warning against the manner in which the idea that the inertia of the water does not resist the ship's way is being given practical application by our shipbuilders, may be allowed to appear in the report of the discussion.

25th March, 1905.

The question dealt with in my letter of 22nd instant is of such importance that I think it desirable to submit some diagrams for the purpose of making the points at issue quite clear.

Fig. 1*a* shows two midship sections which, according to statements connected with plate v., fig. 5, in a paper read by Mr. R. E. Froude on the 24th March of last year before the Institution of Naval Architects, I understand to have been found to be subject to the same resistance. According to my argument that shows the increased lift from the area

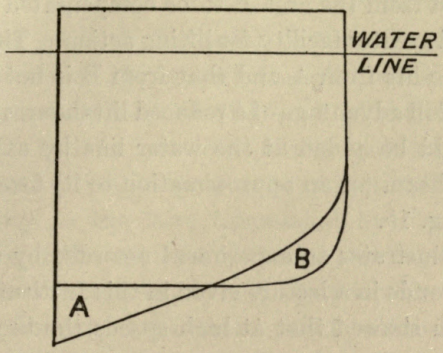


FIG. 1*a*.—Half-Section.

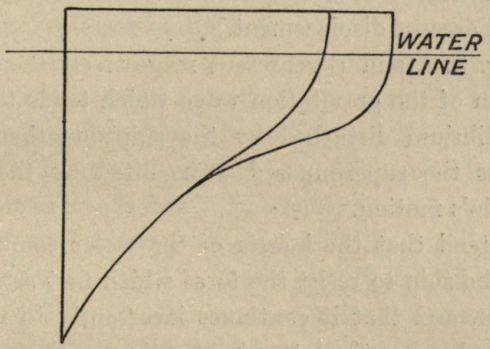


FIG. 1*b*.—Half-Section.

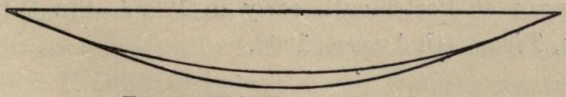


FIG. 2.—Half-Plan at Water Line.

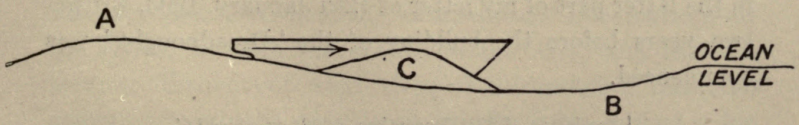


FIG. 3.

A instead of from the area B to be compensated by reduced friction and greater facility for lifting action. The difference between the lift from A and that from B is not very great. To get the full advantage of a reduced lift the area substituted for B should be added at the water line by extending the maximum beam (or an approximation to it) fore and aft, as shown in fig. 1*b*.

Fig. 2 illustrates an experiment recorded by the late Mr. William Froude in a lecture given in this Institution in May, 1876, which showed that at high speeds the broader beam, though increasing the displacement, made considerably the least resistance. That I treat as being due to the decreased height of lift more than counterbalancing the increased area of displacement.[1]

In fig. 3, A and B represent respectively the crest and depression of the gravitation wave which tends to readjust the equilibrium disturbed by the ship's motion; and C represents the superimposed wave lifted by the pressure of the ship's motion.

I contend that the inertia of the water combines with that of the ship to resist the force which first sets the ship in motion, and that it continues incessantly to resist that motion as long as the latter is in action.

'The work of "preparing to build" the "Dreadnought" was begun at Portsmouth early in 1905.'—Sir William White, *Times*, 4th August, 1908.

The following letters show how the correction of the erroneous principle adopted by Stokes and Froude originated in the latter part of my letter of 12th January, 1903, written two years before the building of the 'Dreadnought' was commenced.

[1] See letter of 21st December, 1903, on page 144.

12th January, 1903.

My letter of 12th November last commented on the official attitude of the Institution of Civil Engineers as regards the reformation of the laws of motion which I advocate. I had in previous letters pointed out how, whilst the Institution are giving the weight of their prestige and authority to the laws formulated by Newton under the guidance of the opinions of Galileo and Descartes, some of the most renowned members of the scientific world have practically discarded the main point which I publicly attacked in 1866.

I wish now to point out that Professor Hele-Shaw, who is not only a member of the Institution of Civil Engineers, but is also Professor of Engineering in University College, Liverpool, has in a series of lectures just recently delivered in this Institution treated the theory of motion in accordance with the reformed method I have advocated, tacitly ignoring that part of the so-called First Law of Motion which I have pointed out to be obsolete. He lucidly showed how obstructions on a road prevent wheels from following the straight line in which they tend to move, and that motion is spent because work is done in overcoming the obstacles which prevent motion in a straight line; and afterwards, rolling a wheel in a circle, he said: 'The wheel continues in motion until the energy given to it is exhausted: that is the First Law of Motion.'

It is mere matter of fact that the views expounded by Professor Hele-Shaw throughout his lectures constitute an abandonment of Newton's First Law of Motion and an adoption of the law which I have substituted for Newton's. The fact that the title of the lectures was, 'Locomotion—on the Earth, through the Water, in the Air,' and that the lecturer throughout kept clear of astronomical questions cannot properly be considered to affect the subject; for,

though locomotion in interstellar space involves other conditions under which the law has to act, ' the First Law of Motion,' whatever it may be, must of necessity be the same First Law independently of the conditions under which it may have to act.

According to Newton's law the orbital motion of the earth is ceaselessly doing the mighty work of overcoming the ceaseless effort of the sun's gravitation to draw the earth down to the sun's surface. That effort of the sun's gravitation is the ' obstacle ' which prevents the earth from following a straight line, and compels it to move in a curve ; but nevertheless, according to Newton's law, that immense amount of work is done every instant by the earth's motion without renewal of motive energy, and without reduction of the amount of energy of motion imparted under conditions no longer existent ; whereas according to the law expounded by Professor Hele-Shaw the work done by the earth's motion must involve expenditure of the motive force, which must therefore gradually be spent unless ceaselessly renewed in proportion as work is done by it. That ceaseless renewal of motive force is, I say, supplied by the transverse action of the sun's gravitation, of the existence of which Galileo, Descartes, and Newton were unconscious, and, therefore, invented a law of motion to supply in theory the incessant force found to be practically necessary to maintain the earth's motion. I have (in a paper on ' Vis Inertiæ and Momentum,' offered to the Institution some years ago and since published in my work ' Essays in Illustration of the Action of Astral Gravitation ') so fully commented on the circumstances which led to the invention of that so-called First Law of Motion, that I will not now repeat arguments, but pass to a new aspect of the subject.

Though Professor Hele-Shaw completely ignored the

obsolete idea of any body in the material universe having an innate tendency to continue in perpetuity any motion imparted to it by an initial push or impulse, he nevertheless, in one of his lectures, expounded a commonplace phenomenon in a manner which is a survival of a method of explanation introduced under the influence of that obsolete idea.

In an exceedingly interesting and lucid lecture on 'The Self-propelled Ship,' he made the threefold division of the resistance to a ship's motion—(1) skin friction; (2) wave-making; and (3) eddy resistance, which was formulated by William Froude, who will hereafter perhaps be recognised as the last thoroughly earnest and uncompromising champion of that part of Newton's law which I have declared to be obsolete. His thorough belief in that idea as an active principle in nature led him to endeavour to direct in accordance with it the designing of the forms of ships best suited for moving through the water under its action. Thoroughly believing that fancy to be a really effective and controlling action in interstellar space, he exerted his ingenuity to the utmost to utilise the principle in devices for propulsion through the water.

I do not say that there is anything intrinsically wrong in that threefold division of resistances, provided they are not treated inconsistently with the fact that they are subdivisions of the action by which the inertia of the moving material resists the force by which it is moved; and certainly Professor Hele-Shaw was not in his lectures misled into what appeared to me any mistake by adopting Froude's method; but Froude systematically ignored the fact that the 'waves' and 'eddies' he deals with are the motion by which the water makes way for the ship. The inertia of the water, which as it is forced aside to make way for the ship forms waves and eddies, is expressly denied by Froude to constitute a resistance to the ship's way

('Proceedings of the Royal Institution,' May 12th, 1876, p. 211). He is enabled to ignore the resistance due to the inertia of the water only because he divides it under three separate heads and ignores the fact that those three resistances are consequences of the ship having to force the water aside to make way for itself, and that they are effective actions in the effort by which the inertia of the water resists the effort made to move it. Work is done in moving the water to make way for the ship, and motive force must be expended in doing that work. But Froude not only believed the earth's motion to be perpetuated by inertia, but endeavoured to contrive a utilisation of the idea in the construction of ships. Liberal arrangements by the Admiralty for practical experiments with models, together with joint responsibility of colleagues, has probably prevented some strange shapes from being launched from our building yards whilst Froude was endeavouring to establish his idea; and no doubt, in spite of unsuccessful attempts to develop erroneous theory, useful work has been done, and correct views on some points not previously recognised have become established. About seven years before the date of Froude's above-mentioned paper I was criticised in Buenos Ayres for removing outriggers from a London wherry and fixing them on a Rob Roy canoe for a race in which I undertook to pull a pair of sculls against a four-oared gig over a course of twenty miles. The argument against me was that I was taking a slower boat; but my counter-argument was to the effect that though the wherry was the best for a short race, no man could pull it for twenty miles at the average speed at which he could pull the Rob Roy; and I notice that my view on that point is elaborately justified by Froude's argument in connection with diagram No. 22 in his paper. At any rate, I pulled over the course in the Rob Roy in less time than I consider I could have done in the wherry.

The idea that one initial impulse can be the cause of perpetuity of motion in interstellar space has doubtless led to hours, days, and years having been often spent in the endeavour to contrive a similar perpetuity of motion by mechanical arrangements in terrestrial phenomena by arguments perhaps in some cases sound, supposing their basis to be sound, and Froude's arguments have originated in that same false basis.

I again urge on the Council of the Institution of Civil Engineers that the question I have raised regarding the First Law of Motion is one of vast importance in engineering science. And I also urge that this letter must properly be considered to originate in that of 21st December, 1900, which was written for the immediate purpose of pointing out that the true cause of the accident to the Great Northern Express, which had just then been the subject of a Board of Trade report, had been missed by those who framed that report.

24th January, 1903.

In your letter of 20th instant you allude to mine of 12th *idem* as generally reiterating views expressed in previous communications to which answers have already been made under the Council's instructions. I have already, under date of 21st instant, replied to some points of your above-mentioned letter, and now earnestly urge upon the Council some observations in protest against the incorrect statement I have specified, and which is the really important point of your letter.

It seems to me impossible that anyone could read the letter to which you reply without recognising that the point which it professes to treat as of the greatest practical importance is the exposition it gives of the error at the basis of the theoretical principles by which William Froude

was guided in designs which more or less determined the lines of many ships of our Navy whilst the influence of the views which he introduced predominated in the Admiralty.

You profess to be refraining on your sole responsibility from submitting that letter to the Council, on the ground of the views it expresses having been previously replied to by them ; but the fact is that the exposition there given has never before been offered to you either by me or by anyone else. The Council have never replied to it, and have never had an opportunity of considering it except that given by my letter of 12th instant.

Froude's influence in the Admiralty originated in his opposition to the reformation of the laws of motion which I suggested, and the shapes of many of our battleships and cruisers are a testimony of the error of that part of the so-called First Law of Motion which he endeavoured to make available for practical purposes after I had declared it to be obsolete as a theory and false as a practical principle in all mechanical action.

Every ship now afloat in our Navy constructed, under the guidance of Froude's ideas, with 'hollow lines' is a practical demonstration of the fact that Newton's definition of inertia, which Froude endeavoured to make practically effective, is not a true principle in nature.

21st October, 1903.

As I am confident that the attitude of the Council indicated by your letter of 3rd July is due to a conviction on their part that I am wrong as regards the main point of the question I have urged upon them, and as I am also confident that they cannot be imbued with any desire to refrain from allowing fair play to new views even when

clashing with theory long accepted as a basis for the discussion of engineering questions, I therefore consider it a duty to myself and to the Council, as well as to the true interests of engineering science, to disregard their request and to reinforce my argument to the effect that every ship in our Navy recently designed under guidance of the principles which led to the form to which some of our admirals have recently applied the term ' hollow-lined,' constitutes a practical demonstration to the effect that what is now accepted in all engineering text-books as the First Law of Motion is not true to nature.

The guiding theory which has been followed as much as practical experiments with models permitted not only discards the previously accepted basis of discussion to the effect that the work to be done by a ship, sailing or steaming, is to excavate a passage for itself along the surface of the water, but also, instead of that idea as a basis for treating the subject, the recently adopted theory is based on the idea that ' a fish moving at a steady speed through frictionless fluid would experience no resistance at all. In fact, when once put in motion it would go on for ever without stopping.'

I am not at all surprised that anyone who thoroughly believes that the earth has been for thousands of years moving round the sun at the same velocity, in consequence of an initial impulse, without any force being now incessantly in action to sustain the motion, should attempt to utilise that idea in the manner indicated in the above quotation.

It is argued that the inertia of the water offers no resistance to the progress of the ship, because the head resistance as the water is pushed out of the ship's way is balanced by the stern pressure of the water refilling the vacuum. Granting the argument to be correct as far as the fore and aft pressures are concerned, I say that,

even in the assumed frictionless fluid, the stern pressure represents the force which propels the ship, and any reduction of that propelling force leads to a corresponding reduction of the stern pressure. The force with which a stone lifted vertically strikes the ground on its return is equivalent to the force exerted to lift it ; but there must be fresh expenditures of lifting force to enable the stone to continue striking ; and so also the force which any drop of water falling back to its place exerts as stern pressure must first be supplied by expenditure of force exerted in lifting it from its place.

The protests which have come from practical seamen against the shapes of some of our new ships show that for the comfort of our sailors at all times, and for the accuracy of our fire when fighting, the dominant idea by which the Admiralty have been recently guided, and are still avowedly guided, in naval construction ought to be abandoned ; and it appears to me that those principles are also erroneous when considered only as a means for obtaining a maximum of speed with a minimum of resistance.

The term ' venom of pitching ' was probably unknown in the Navy before William Froude and Scott-Russell answered my attack on the First Law of Motion by giving it more practical application in naval construction than had ever before been attempted.

21st December, 1903.

As I am confident that the following arguments will be found to justify me in expecting that the Council will change from the attitude reported in your letter of 27th November as regards my effort to impress on them evidence to the effect that the credit of engineering science and the interests of the country demand a radical change in the theoretical principles by which the Admiralty have in

recent years been guided in naval construction, I urge them on your attention, premising that it has been, I presume, understood that I do not pretend to determine precisely the shapes which correct principles require. William Froude (in the paper referred to in my letter of 12th January) admirably comments on the point I am bringing to issue when he says : ' It is easy to imagine how fruitful, in false aims and false principles of nautical construction, would be the assignment of the resistance of ships to a supposed cause which has no existence at all.' [1]

It must, I think, be admitted that it may be equally disastrous to design the shape of a ship on the supposition of the non-existence of a resistance which in fact does exist ; and that, I say, is what has been done during the past quarter of a century and is now avowedly being persisted in. The merits of the point at issue depend on the question as to whether the inertia of the water through which the ship moves does or does not cause it to resist being displaced to make way for the ship. That main point I have directly dealt with in my letters of 12th and 24th January and 21st October, and I will now support that argument with the following points.

First, in the paper above referred to, Froude says : ' I am but insisting on views which the highest mathematicians of the day have established irrefutably, and my work has been to appreciate and adapt those views when presented to me.'

I agree that if the inertia of matter really tended to perpetuate motion in accordance with the axiom adopted by the mathematicians above alluded to, it might be mathematically demonstrated that the inertia of the water through which the ship moves does not resist the ship's way ; but

[1] What Froude alluded to as having no existence at all is the resistance caused by the inertia of the water.

I urge that, in fact, the logical application which has been made of that principle in naval construction constitutes a practical refutation of the principles on which those mathematical demonstrations have been based.

In diagram 23 [1] Froude gives water-line plans of two ships of the same length, but one with more beam than the other; and he asserts that 'on the midship section theory this one would clearly have the greater resistance of the two.' The context shows that the illustration is given in contravention of the idea that the resistance is due to the inertia of the mass of water which has to be displaced. That mass is identical with the mass of the ship; and I say that the question at issue in those two shapes is dependent not only on the mass and velocity of the ship, but also on the relative amount of work which has to be done to get equal masses of water out of the way of ships of different shapes moving at the same velocity.

Mr. Froude continues: 'Nevertheless, in the trial of two models of those lines it appeared that at the higher speeds the form with the largest cross section made considerably the least resistance.' And he then argues that 'the explanation of this lies of course in the fact that the addition amidships, though increasing the displacement, forms a prolongation of the wave features at the two ends, and thus lessens the wave-making resistance.'

In reply to the above, I say, in the first place, supposing the two ships to be of the same displacement, the broader beam offers least resistance, because the displacement is closer to the surface and therefore more easily effected. And, secondly, it is an interesting and important question to determine to what extent the greater facility of displacing water near the surface than deeper will admit of a greater

[1] See fig. 2, diagram page 135.

tonnage being moved at equal velocity without greater resistance. It seems to me that the 'increased displacement' referred to by Froude cannot have been very great. And I say the evidence of the experiment really tends to show that the ship's motion causes a wave only because that is the easiest method for the water to get out of the ship's way. The deeper the displacement, the greater is the height through which the water has to be lifted to make way for the ship. This seems to me to have been exemplified by the manner in which 'Reliance' beat 'Shamrock III.' in all weathers. I have not found any practical experiment recorded to show that resistance is greater at the surface than deeper, as asserted by Froude and others, and in the absence of such demonstration it seems to me that the idea ought not to be relied on as a fact on which to base argument. The wave rises to permit the ship to advance, and if the ship could progress more easily without raising the wave, the wave would not be created. As water is practically incompressible under the conditions we are discussing I do not understand how it can be supposed to make way for the ship (whether totally or only partly submerged) without rising in some form of wave. It seems to me that mathematics which demonstrate the possibility of so shaping a ship as to avoid the creation of a wave must of necessity be based on some principle which is not true to nature.

Other circumstances which justify the insistence of my appeal to the Council of Civil Engineers are that the result of the recent discussion in the Institution of Naval Architects was a declaration by the President to the effect that Admiral Fitzgerald, who protested against 'hollow lines' as practically disadvantageous, had been defeated; and the discussion shows no intention of changing from the theoretical principles by which William Froude, Scott-Russell, and Nathaniel Barnaby were guided in designing

the shapes against which Admiral Fitzgerald protested. Admiral Fitzgerald's protest is based on his practical experience at sea on board the ships. He simply declares the shape of the ships to be bad without entering on the question as to whether that is due to erroneous principles of construction or to faulty treatment of scientific principles themselves intrinsically faultless. My argument is, on the other hand, merely to the effect that the principles by which the Admiralty have avowedly been guided are erroneous. And that is a question independent of that as to the extent to which tank experiments have prevented practically disastrous shapes from being constructed. The argument I am urging is untouched in the discussion of the naval architects or anywhere except in the appeal I have made to the Council of the Institution of Civil Engineers.

I beg, however, to be allowed to point out that the above-mentioned discussion seems to have turned more upon what is a trivial point than on the main question even from Admiral Fitzgerald's point of view. It might be supposed from the tenor of the discussion that all at issue in the protest was about fifteen inches of width on each side of part of the bows of those huge ships. But, in fact, that is merely the extent to which at the water-line the bow curves inwards. Lower down the inward curve is more than three feet; and the real question at issue is as to whether the bow ought not to bulge *outwards* two or three times as much as it is actually made to bulge inwards.

The question really raised by Admiral Fitzgerald is, therefore, as to whether there is not a width of twenty-five feet *outside* some parts of the bow which might advantageously be brought inside by constructing a bluff instead of a hollow lined bow. That is to say, the question is as to whether the line from the cutwater to the full beam of, say, sixty-six feet is in some part to place a width of twenty-five

feet inside or outside the ship; and that constitutes an important area even in the largest ships. That question does not, however, affect my argument as to the merits of the theoretical principles which the Admiralty are endeavouring to make effective; and the fact of the discussion by the Institution of Naval Architects resulting in favour of the maintenance of the principles of which Froude was essentially the champion demands persistence in the crusade against them.

The Council I am addressing are pre-eminently qualified to deal with the twofold question which I have propounded, and I trust the immediate reason given in previous letters for appealing to them will not be ignored.

13th April, 1904.

In my letter of 21st December last I urged, in justification of my insistence in addressing the Council of the Institution of Civil Engineers on the subject of naval construction, the fact that the last discussion by the Institution of Naval Architects had resulted in favour of the maintenance of the principles of which the late William Froude was essentially the champion.

A series of lectures which Lord Rayleigh has recently delivered in this Institution on 'The Life and Work of Stokes' constitutes a further demand for persistence in my crusade against those principles.

Lord Rayleigh claims that it is only by practical application of the mathematical work of Stokes that Froude and others established the principles of naval construction which have been adopted by the Admiralty. The whole tenor of the argument in which Lord Rayleigh claimed credit for Stokes for the principles by which the Admiralty have been guided in designing the shapes of recently built

ships, is a thorough confirmation of the argument of my letters of 12th and 24th January and 21st October, 1903, as far as concerns my statement that ' Froude's influence in the Admiralty originated in his opposition to the reformation of the laws of motion which I suggested.' Stokes considered my opposition to the ' First Law ' to place me outside the pale of reasonable science ; and Froude evidently considered that a mistake had been made, but that it consisted, not in any error in the laws of motion, but in never having given the First Law its due practical application in designing the shapes of ships. It appears to me that Lord Rayleigh is quite correct in claiming that the designs of our recently built ships have been based on the mathematics of Stokes ; and no scientific man versed in the subject will, I think, dispute that in the principles which Stokes represented and acted on, that First Law was regarded as an equally reliable axiom as the statement that two and two are four.

The point I wish to enforce is to the effect that the shapes of the ships under discussion are the result of an attempt on the part of practical men to put into practice principles demonstrated by pure mathematicians to be correctly based on what they suppose to be incontrovertibly the true laws of motion. Lord Rayleigh has, in fact, fixed on Stokes the responsibility, according to the tenor of my letters above mentioned, for ' Shamrock III.' having been so shaped as to give her no chance in any weather against ' Reliance.' It seems to me that the American builder was guided by experiment rather than by theory in changing from the shape of ' Defender,' and perhaps did not clearly recognise the precise theoretical points which made his new boat superior to ours.

I wish to avoid repetition of arguments already laid before you ; but I am able to reinforce those arguments by

the evidence of a practical experiment recently made in the Lecture Hall of this Institution.

On a day intervening between two of the above-mentioned lectures by Lord Rayleigh, Professor Trouton delivered a lecture in which he exhibited an experiment in illustration, professedly, of the manner in which Stokes's mathematical inductions from the laws of motion had been applied to shipbuilding as described by Lord Rayleigh.

In the diagram (page 152) A and A′ are two similar glass vessels containing liquids; the liquid in A having greater specific gravity than that in A′.

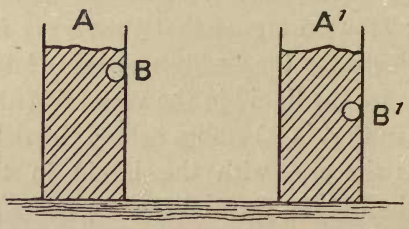
B and B′ are two bubbles started at the same time from the bottom of the respective vessels.

Professor Trouton argues that the slower motion of the bubble B′ illustrates the 'eddy resistance' to the motion of ships, because the liquid in the vessel A′ (which is in fact less dense than that in A) eddies behind the rising bubble B′ more than is the case with the liquid in the vessel A as regards the bubble B; and that in consequence of the greater eddy resistance the upward motion of B′ is more retarded than that of B.

I must say it appears to me that Professor Trouton is perfectly justified in offering that experiment as an illustration of Froude's theory of eddy resistance to the motion of ships; but I beg that, as practical men, the Council of our Institution will recall to mind Torricelli's original explanation of the rising and falling of the common barometer, and recognise that the principle then established naturally requires the bubble B to rise faster than B′ simply because the liquid A being more dense than A′, a greater force is applied to propel B upwards than is applied in the same manner to B′. The force of propulsion is the amount by which the earth's gravitation acting on a volume of liquid equal to that of the bubble exceeds that acting on the

bubble ; so that the propelling force acting on B exceeds that acting on B′ exactly as the density of the liquid A exceeds that of A′.

There doubtless is some difference in the formation of eddies behind the two bubbles, due to what Lord Rayleigh terms viscocity, and which I treat as chemical affinity, whether a bubble or a ship is under discussion (friction being as the degree of chemical affinity) ; but in giving that as the cause of the difference in the velocity of their motion, theoretical guesswork, based on theoretical action of theoretical laws, is being substituted for a plain matter of fact explanation which had been given long before the



theoretical laws or the still more clever and elaborate mathematics based upon them had been applied to naval construction. I have, in previous letters, pointed out that in the practical work of naval construction Froude has made many of our finest ships practical proofs to the effect that the principles accepted by Stokes are not true to nature ; and it seems to me that Professor Trouton, in his explanation of the above-mentioned experiment, has very cleverly brought those principles to a *reductio ad absurdum*.

In face of the influential position of the Council of the Institution of Civil Engineers, a great responsibility will rest on them and lessen the high estimation in which they

have deservedly been held if in spite of the facts and arguments I have submitted to them any more vessels are built in our dockyards under the principles of construction which Froude based on the mathematics of Stokes. But a responsibility of a quite different and of a still more serious character will be incurred if all members of the scientific world remain impassive whilst the unfair conduct against which I have protested is persisted in whilst the Admiralty abandon the false principles of Stokes and Froude and act on those for advocating which I have encountered the hostile action described in my letters of 29th April, 1901, 17th May, 1901, 3rd November, 1902, 25th February, 1903, and 12th May, 1903.

9th January, 1905.

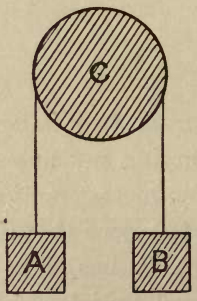
I have received from two independent sources fresh supplies of fuel for urging my appeal to our Council, as representatives of the most practical men among those prominent in science, to rescue the scientific world from the predicament in which it has been placed by those in possession of the Royal Society.

First, recent lectures in this Institution : A course of six lectures has just been given on 'Measuring Time'; but their main purpose was, at the outset of the first of them, avowed to be to impress on the minds of a juvenile audience the truth of the laws of motion declared to have been discovered and irrefutably established by Galileo and Newton.

One of the two phenomena declared to be a practical proof of the so-called First Law was shown by 'Atwood's machine,' which has been utilised for the purpose for more than a hundred years.

The point essential to this question is that equal weights,

A and B, are suspended over a pulley C ; a slow downward motion is artificially given to B, with corresponding upward motion to A ; and then, because the motion so created is continued after the action of the force by which it was started is removed, the juveniles are told (as the lecturer was himself told when he was a juvenile) that they see before them a practical demonstration of the truth of the law which declares that a body once set in motion tends to persist for ever in that state of motion. The glaring facts of the case are that, if the weights are really in equilibrium when on a level, as in the diagram, then any fall of B and rise of A must make B



heavier than A, because the respective weights are the respective forces of gravity acting between each of them and the earth, so that the motion increases the weight of B and decreases that of A.

The fact is that motion is continued in the above experiment simply because each quantity of motion effected brings into action a fresh amount of force for the creation of a further quantity of motion, even without considering the weight of cord transferred from one scale to the other as the motion is effected.

The motion continues because motive force is continuously supplied.

The other experiment in asserted demonstration of that so-called First Law was a dummy passenger placed in an open railway truck ; and on the truck being suddenly stopped by an obstruction on the rails, the passenger went head first forwards out of the truck. As far as exemplifying that first law is concerned, a bullet might have been fired from a rifle or flung from a sling, and the motion beyond the barrel of the rifle, or after leaving the sling, declared a proof of the law.

The fact is that in each case a quantity of motion proportioned to the motive force exerted on the propelled body is effected and motion ceases as the force is spent. For continuity of motion there must be continuity in the supply of motive force.

A phenomenon alluded to which really does bear on the above question, though it was not treated as being connected with the subject, is the fact that to make an ordinary pendulum truly isochronous with changes of amplitude its path must be made to describe a segment of a cycloid instead of that of a circle.

The only explicit explanation that has ever been given of that fact is that the action of astral gravitation resists all motion with a force which is as the square of the motion resisted ; and the slow motion in the short vibrations of the pendulum is therefore less resisted than the faster motion in longer vibrations. To compensate for that decrease of resistance in short vibrations a seconds pendulum has to be made about half an inch longer if used in them than if used in vibrations of 90° (as shown in my letters of 7th May and 21st November, 1903).

The existence of that resisting action makes it impossible for continuity of motion to exist in the universe without continuity in the supply of motive force ; and the so-called First Law is therefore useless as an explanation of any

existing continuity of motion. Some other cause must in each case exist.

The second source to which I have alluded is a series of letters recently addressed to the *Times* by Sir William White on the United States navy.

Sir William White tells us that United States battleships of the 'Vermont' class displace, when fully loaded, 17,650 tons and draw 26¾ feet; and ours of the 'King Edward' class displace 17,500 tons and draw about 28½ feet when similarly equipped.

That difference between our battleships and those of the United States is exactly what I suggested (in my letter of 21st December, 1903) as being probably the difference between our yacht 'Shamrock III.' and the American 'Reliance,' which gave the latter the superiority both in steadiness and speed.

I am not going to rewrite the arguments of my letters of 12th January, 1903; 24th January, 1903; 21st October, 1903; 21st December, 1903, and 13th April, 1904, to the effect that all our hollow-lined cruisers or battleships now afloat are practical demonstrations to the effect that the so-called First Law of Motion is not true to nature, but merely recapitulate to the effect that up to the time of my attack on that law, its direct application to shipbuilding had never been attempted. The late William Froude then, instead of rejecting the law, as I proposed, gave it practical application under guidance of the mathematical demonstrations of Sir G. G. Stokes and others, and the result which now becomes evident is that our battleships have to lift the water from nearly two feet of greater depth to make way for themselves through the ocean than has to be done by United States ships of the same class.

Our ships are condemned to do that greater depth of ploughing merely because, under the guidance of Stokes's

mathematics, Froude was convinced that the *inertia* of the water does not resist the ship's way.

More work has to be done to lift water 28½ feet than to lift it 26¾ feet; and I leave it to Sir William White to say if the shapes of our ships give them any compensating advantages.

It is time for the Royal Society to recognise that the rejection of my paper on the tides in the year 1868, under the guidance of Sir G. G. Stokes, was a blunder; and Froude's action in applying the so-called First Law to naval construction after I had denounced it was a natural sequel to that blunder.

That law was introduced into science for astronomical purposes; and until Froude applied it to naval construction I do not think any attempt had been made to give it practical application in terrestrial physics except by those whose thorough acceptance of it as a firm theoretical basis for practical mechanics misled them into schemes for endeavouring to contrive perpetual motion by the application of a merely initial force. The resistance of astral gravitation, which limits the quantity of motion to the quantity of motive force, baffled all those clever contrivances.

Sir William White is among those to whom this letter is addressed, and there is no one more competent than he is to decide as to whether the cost of converting some of our ships from hollow-lined to buff bows would or would not be compensated by the advantages of increased steadiness as fighting platforms and increased speed with equal motive force.

30th October, 1906.

In a leading article yesterday the *Times* raises the question as to how far the 'very high speed' of the 'Dreadnought' may have been obtained at a cost in excess of

its value as estimated in terms of fighting efficiency ; and then says, ' The design of the " Dreadnought " is not the outcome of any single man's ideas.'

I beg most respectfully to urge on the Council that the letter I addressed to you on 12th January, 1903, is the origin of the change from the shape of the ' King Edward VII.' to that of the ' Dreadnought.'

My said letter of 12th January, 1903, and those which followed it on the same subject gave a death-blow to William Froude's ' fundamental Principles of the Resistance of Ships,' by which the Admiralty were guided in the construction of the ' King Edward VII.' and all our other battleships and cruisers then in commission ; and the Admiralty are now shaping our ships on new designs based on the mathematical principles advocated in the letters I have addressed to our Institution.

That new mathematical basis has virtually been entrusted by me to the Council of the Institution of Civil Engineers, and I appeal to them not to acquiesce in its appropriation by highly paid official experts without any recognition of the ' amateur ' to whom they are indebted.

STEEL RAILS

24th February, 1906.

A lecture delivered last evening in this Institution by Professor J. O. Arnold, of Sheffield, constituted an elaborate practical demonstration of the correctness of the main point of the second section of the paper which I offered to the Council of our Institution on 21st December 1900.

Through eight pages of argument I challenged the then prevalent idea to the effect that lines of cleavage in the crystals of any metal are a source of weakness, and against that view I suggested that ' the crystal growth is the effort

of gravitation to give the metal its strongest possible conformation, and those views indicate that the effort of manufacturers should be to create uniform crystallisation, so as to make one homogeneous crystallisation pervade the whole rail. The lines of cleavage doubtless are, when homogeneously extended, a source of elasticity in the metal, but I contend that they nevertheless give the strongest bond of union of which the atoms of the metal are susceptible.'

The experiments explained by Professor Arnold are a clear and splendid confirmation of the theoretical argument which I submitted to our Council more than five years ago.

26th March, 1906.

James Mansergh, who was President of our Institution when the Council rejected my paper of 21st December, 1900, has gone out of reach of the appeal for reconsideration which is overwhelmingly justified, as far as I am concerned, and at the same time urgently demanded from the point of view of the ' interests of the Institution ' which the Council, by your letter of 15th December last, have thought proper to inform me it is their object to promote.

Seldom, in the short space of about five years, has a purely theoretical argument been so splendidly confirmed by practical experiments as the main argument of the second part of my above-mentioned paper has been by the demonstrations alluded to in my letter of 24th February. In face of the practical experiments recorded by Professor Arnold 'the interests of the Institution ' clearly call for a change of attitude on the part of the Council as regards my paper of 21st December, 1900. The difficulties inseparable from new and abstruse subjects must often be held to excuse misappreciation of papers offered to the Council ; but Professor

Arnold's lecture in this Institution, referred to in my letter of 24th ultimo, creates a perfectly new position and gives the theoretical action on which that paper is based more importance, and fairly entitles it to greater consideration than if the paper had been written for the purpose of explaining the facts discovered by Professor Arnold after they had been recorded.

If there were such things as mathematical anomalies, the gravitation formulæ recorded in my letter of 18th January might be treated as such. The fact, however, that 16·1 as the foot-pound-second equivalent of force in gravitation measure gives, as representing g, a complete set of simple formulæ which is not given by 32·2, proves the latter figure to be false. And, moreover, it must be borne in mind that I, in the first instance, pointed out that the movements of the planets show 16·1 (and not 32·2) to be the true figure, and the subsequent evolution of the formulæ simply confirms the antecedent demonstration to that effect.

10th April, 1906.

Your letter of 5th instant has only just reached me, having apparently been posted last evening, leaving me only just time to reply before the closing of the session to-night.

As no modification of Newton's gravitational theory can be said to have become generally accepted, and as for forty years there has been no more thorough supporter of the Newtonian law of gravitation than I am, your letter creates a false issue whilst it ignores points really in my correspondence with which it is impossible for anyone to deny that engineering considerations are concerned.

What gives a cynical aspect to the issue created by your letter is the fact of Sir Alexander Binnie being not only

President of the Institution of Civil Engineers, but also a member of the Committee of Managers of this (the Royal) Institution. He is therefore one of those to whom letters addressed to Sir William Crookes as Secretary of this Institution are in fact addressed; and the following comment on a lecture delivered in this Institution last May is therefore addressed to him. In a letter dated 15th May I wrote: 'The lecturer's argument to the effect that Newton's law of gravitation requires modification shows him to be completely on the wrong track.'

I beg that you will kindly submit this letter to the Council this evening.

16th November, 1906.

I have read with much pleasure your letter of 14th instant, which seems to offer the new views I have advocated on several subjects connected with the engineering profession a new platform from which to assail what appears to me to be generally accepted mistakes.[1]

As regards the last sentence of your letter, which seems to rebuff beforehand my availing of the new position offered, I have already advised you that the editor of the 'Engineer' (*ab uno disce omnes*) told me that if he were to admit into his paper my view to the effect that the so-called First Law of Motion is not true to nature, he would make himself the laughing-stock of the engineering profession, and that, therefore, my only course is to appeal direct to the leaders of the profession of whom that editor and others stand in awe.

[1] Unrealised up to 18th September, 1908.

PART III

LETTERS IN CONNECTION WITH LECTURES DELIVERED IN THE ROYAL INSTITUTION AND IN THE INSTITUTION OF CIVIL ENGINEERS

NOVEMBER 1908 TO APRIL 1909

IN THE HOPE OF SETTLING
AT ONCE AND FOR EVER THE QUESTION
AT ISSUE IN CONNECTION WITH
THE TIDE WAVES OF THE GREAT SOUTHERN OCEAN
I DEDICATE THIS BOOK
TO
HIS MAJESTY'S MOST HONOURABLE LORDS
COMMISSIONERS OF THE ADMIRALTY

PREFACE TO PART III

The 'Sling Letters' originated with the one, dated 15th May, 1905, written for the purpose of defending the accuracy of Newton's Law of Gravitation against a recent attack.

For some time previously an idea had been spreading in scientific circles to the effect that a modification of that Law had been found to be necessary, and the formal declaration made in the Lecture Hall of the Royal Institution to the effect that Newton's Law could no longer be regarded as accurate was rebutted in the First Part of 'The Sling' so thoroughly that I do not believe there is now to be found a scientific man who any longer considers the attack then made to be valid. It may fairly be said that Newton's Law of Gravitation never commanded the allegiance of science more completely than it does to-day.

Edmund Halley, Robert Hooke, Sir Christopher Wren, John Flamsteed and other leading Fellows of the Royal Society were contributors to the official and unofficial discussions which resulted in the production of Sir Isaac Newton's 'Principia' and its publication by our Royal Society in such a manner as to give it almost the position of a national work; but it should never be forgotten on this side of the English Channel that the ultimate acceptance of the work in the scientific world was directly due to the active intervention of the Government of France. No such abstruse and laborious scientific work had ever before been attempted as was then undertaken and successfully conducted by Maupertuis, Clairaut, and Bouguer to

measure arcs of the meridian in wild regions exposed to the extremes of polar cold and equatorial heat. Their feats of physical endurance, together with a combination of mathematical knowledge and skilful management of scientific instruments, resulted in confirming Newton's argument to the effect that the surface of the Earth must be nearer the Earth's centre in the polar than in the equatorial regions to account for the slower vibration of the pendulum in the latter regions than in Europe.

Any consideration of the gravitation exerted by the stars was excluded from the ' Principia ' on the supposition of their distances being too great to admit of their forces affecting our solar system, together with a further suggestion, to the effect that even if their forces could reach our system their opposing attractions would destroy each other (Cor. 2, Prop. XIV., Book III.) ; but the transverse action of gravitation, which is in fact a corollary to Newton's Law, makes the forces of astral gravitation play their part in every phenomenon of terrestrial physics.

A new illustration of the resistance of astral gravitation to all motion is given in my letter of 20th April to the Institution of Civil Engineers on the Wear and Tear of Roads ; and the story of Perdita given in my letters to the Royal Institution, whether it be found open to correction or not, serves to illustrate the transverse action of gravitation.

Royal Societies Club,
4th May, 1909.

LETTERS

To Sir William Crookes, F.R.S., Secretary, the Royal Institution

29th March, 1909.

Dear Sir,—Mr. Eddington's very interesting paper on Some Recent Results of Astronomical Research which he read last Friday evening in our Lecture Hall carries me back to the letter which I addressed to you on 24th May, 1905 (since published in 'The Sling,' Part I.).

I there gave a reason for saying that, in addition to the known masses of the planets and asteroids, there must be material circulating round the sun equal in the aggregate to not much less than the mass of Saturn; and that it is wanted chiefly between the orbits of Mars and Jupiter inclusive. The evidence for that statement is given by a lengthy analysis of planetary velocities which shows that each time Mars passes between the sun and the giant planet Jupiter the latter makes an appreciable effort to annex the little planet to its system as a satellite.

At the immense distance of 21,000,000 miles from Jupiter, which Mr. Eddington gives as the maximum distance of the newly discovered satellite, J VIII., the sun tends to give it when passing between the sun and Jupiter an orbital motion round the sun about 1,400 feet per second faster than that of Jupiter; and when outside about 1,400 feet per second less than Jupiter's velocity.

I venture to suggest that the new satellite, J VIII., is really another asteroid, which, under the action of the sun's

gravitation, is constantly endeavouring to pass on this side of Jupiter with a velocity 1,400 feet per second faster than the latter. But Jupiter, in the same manner as that in which it unsuccessfully endeavours to change the status of Mars from planet to satellite, does in fact succeed in changing the direction of the motion of the newly discovered J VIII. so that it passes in front of Jupiter, and as its velocity in its solar orbit is retarded it falls back towards the sun behind Jupiter to renew its attempt to get past; the swirl round Jupiter being the latter's perturbation of the solar orbit of J VIII.

Mr. Eddington's statement that 'the orbit round Jupiter is so far from being a closed curve that it is almost meaningless to speak of a definite period of the satellite' seems to me to go far in support of what I have above suggested.

J VI. and VII. are perhaps also asteroids which Jupiter has overtaken and drawn from outside his orbit into the vortex of his own revolving action. All three perhaps intruded suddenly into Jupiter's system as fragments from a planet wrecked between the orbits of Mars and Jupiter.

The foregoing suggestion gives what I may call a *living* explanation of the phenomenon in contradistinction to the *dead-hand* explanation given by Mr. Eddington. When I first gave reason for supposing that the earth has changed its axis of rotation, not once only, but many times, the suggestion was considered sufficient to condemn me and all my works, but now it is treated by some scientific men as if it were quite a triviality. Because the outermost satellites of Jupiter, Saturn, and Uranus revolve backwards, an easy explanation is supposed to be found by inferring that all the planets formerly rotated in that direction, but turned completely over so as to rotate in their present direction

after the above three satellites had been separated from their respective primaries; and that all the satellites now revolving in the normal direction have been separated since that turning over. That may fairly be termed a dead-hand action, as it supposes a motion to have been created in some unknown manner untold ages ago and to have maintained itself ever since.

The dead hand was first called for to explain phenomena for which no *living* explanation seemed available; but I deny that it is at all wanted to explain the backward motion of the outermost satellites of Jupiter and Saturn, as the living explanation serves the purpose and makes them the youngest instead of the oldest satellites of the solar system. Neptune's satellites I leave in quarantine, though I think it probable that the explanation I gave in 'The Ocean,' Chapter XXI., Proposition XXXVII., will prove correct.

2nd April, 1909.

I supplement my letter of 29th ultimo for the purpose of recording some indications given by a lengthy array of old calculations which have an important bearing on the question as to whether the newly discovered satellite of Jupiter has always belonged to Jupiter's system, or is really a newly discovered asteroid whose normal solar orbit is smaller than Jupiter's, but so close to the latter that the giant planet prevents its little neighbour from passing on and making a shorter orbital period than its own.

It would be almost as much out of place for me now to discuss the question as to the existence of a transverse action of gravitation around the sun as to discuss the question as to whether the sun exerts any direct force of gravitation. The latter is in fact inversely as the square, and the former inversely as the distance from the sun;

and therefore, though near the sun, the direct vastly exceeds the transverse force of its gravitation, the ratio of the latter must gradually increase with increase of distance until at length it becomes the greater of the two forces. This, however, applies only to the plane of the sun's rotation, for directly above the poles the transverse force is non-existent.

Some calculations which I made a few years ago make the maximum distance at which the sun's gravitation can control its own transverse action about 15,460 lunar radii (that is 238,800 miles × 15,460). At 20,000 lunar radii from the sun its transverse action in the plane of its equator is to its direct force of gravitation as 1 is to 0·679.

On the same basis, the boundary of Saturn's system is 1,000 lunar radii from Saturn ; and that of Jupiter more than 1,000, and less than 2,000, say about 1,500 lunar radii from Jupiter. The latter would therefore be well able to control its newly discovered satellite at the comparatively short radius of 21,000,000 miles and to make its own transverse action effective in that orbit if it were not thwarted by the greater force of transverse action exerted by the sun. The latter is predominant, but Jupiter's force must tend to throw the plane of the orbit of the asteroid satellite out of the plane of the sun's rotation, whilst at the same time the sun's action throws it out of the plane of Jupiter's rotation sufficiently to make the transverse action of the latter relatively ineffective at the actual distance.

5th April, 1909.

A question which suggests itself in connection with my letters of 29th ultimo and 2nd instant is :—Where is Perdita ? The asteroids do not suffice to account for more than a thin outer crust wrenched to fragments by opposing

tidal forces, and continuing for the most part to revolve near the planet's former orbit.

G. H. Chambers records that the German astronomer Olbers suggested that those asteroids might be fragments of a large planet which had, by some catastrophe, been shivered to pieces ; that Le Verrier says they cannot equal more than one quarter the mass of the earth even if three thousand of them exist ; and that, when only four fragments had been discovered (Ceres, Pallas, Juno, and Vesta) Thomas Campbell believed that thirty thousand more remained to be discovered.

The outer crust of Perdita may have become insufficiently pliant, whilst also not strong enough to resist the tension between the sun and the four giant planets on the latter being in conjunction ; but was there no central part hard enough or tenacious enough to hold itself together and survive the storm which wrecked its surface ?

Edmund Halley, who may metaphorically almost be said to have invented Sir Isaac Newton, and in fact was the first champion of the ' Principia,' was convinced that below the earth's outer crust there is a nucleus which rotates faster than the surface which rests upon it ; and I think that I have supplied arguments even stronger than Halley's own in support of his suggestion. Is such a nucleus likely to have fallen into the sun, to have been flung out of the solar system into space, or to have crumbled into dust to form a meteoric ring circling round the sun ? If none of those three suggestions be valid, then, where is Perdita ? Will not the resources of the science of which you, Sir William Crookes, are a veteran representative enable you to trace her course from the orbit in which she was wrecked to her new position ? If not, then I invite you to consider how the transverse action of gravitation may enable us to trace her startled career and reveal her identity.

Immediately on being released from the drag of the surface torn from it, the rotation of a planet would increase in velocity (as shown by Proposition X., Chapter XXI. of 'The Ocean'), and that increased velocity would cause it to spring away farther than before from the plane of the sun's rotation (as shown by Section 5, Proposition XXXV., Chapter XXI. of 'The Ocean'), where the consequent reduction of the sun's transverse action would cause it to fall rapidly towards the sun, thus separating its course completely from that of the fragments of the wrecked surface.

We may, I think, assume that the sudden increase of the planet's rotating velocity would be followed by a rapid decrease as energy became dissipated by volcanic action no longer suppressed by the superincumbent weight of the outer crust; and the slower rotation would allow it to fall again towards the plane of the sun's rotation to find a new position of equilibrium (as shown in the above mentioned proposition). The question is, Where under such conditions could Perdita have found a position of equilibrium?

An important difference between the tidal action described by Sir Isaac Newton and that indicated by the substitution of the counter-attraction of astral gravitation for his treatment of the action of centrifugal force is made glaring by the irreconcilability of the new view with the argument given in Proposition XXXVIII., Book III., of the 'Principia,' to the effect that the fact of the moon presenting always the same face to the earth must be considered to prove that her figure is that of a spheroid, and that the *longest* axis lies in the radius vector of the orbit; whereas according to the theory of astral gravitation, if the moon is homogeneous, or nearly so, then the axis in the line of the radius vector must be *shorter* than that in the line of orbital motion round the earth, so that her figure is in fact that of

an oblate spheroid of rotation which presents itself to us as a circle because one of the poles of her former rotation is directed to the earth, so that we see (along the edge of her figure) the complete circle of her former equator.

To get that figure the moon must once have been a rotating world—a sister planet of the earth, and not a mere satellite. 'She nightly, to the listening earth, repeats the story of her birth.'

13th April, 1909.

In further support of the idea of our moon being the Perdita which once revolved as a planet of the solar system between Mars and Jupiter (as suggested in my letters of 29th March, 2nd and 5th instant) it is of interest to record that Proctor and Raynard in their 'Astronomy.'[1] called attention to a remarkable distinction between our moon and all other satellites of the system. They pointed out that all the other satellites are in the 'domain' of the planets round which they respectively revolve, but that our moon is not in the earth's domain: that is to say, if we suppose the moon's momentum to vanish and no transverse action of gravitation to exist, then the moon would fall to the sun—not to the earth; whereas all the other satellites then known would, under similar condition, fall to their respective planets—not to the sun. The moon in that sense belongs to the sun—not to the earth; whereas all the other satellites then known belong to the planets round which they respectively revolve.

The moon is, from the above point of view, an outsider in relation to our terrestrial system, and may have been drawn into the vortex of the earth's transverse action from a solar orbit larger than that of the earth as suggested in my letter of 29th March as regards Jupiter's satellites J VI.

[1] In the chapter 'The Moon considered as a Planet.'

and J VII. Those two satellites are within what Proctor and Raynard have happily termed Jupiter's domain; but J VIII. is far outside that domain, though not relatively so far outside as our moon—shall we say Perdita ?—is outside the earth's domain. Those are the only two satellites in the solar system which are outside the domain of the planet round which they respectively revolve. There is, nevertheless, the important difference in their orbital motions that Jupiter's Octavia revolves backwards, in the opposite direction to Jupiter's transverse action, whereas the earth's Perdita revolves forwards with the earth's transverse action.

A living, in contradistinction to a dead-hand explanation of that difference, is available under the supposition that the earth overtook Perdita whilst the latter was moving in a slower orbit outside the earth's, and that the reverse was the case as regards Jupiter and Octavia, as suggested in my letter of 29th March. It is not necessary to capsize Jupiter to explain Octavia's backward motion.

After the wreck of Perdita, Octavia may have revolved round the sun many times in an orbit of her own before chancing to be at the same time as Jupiter at the intersection of the planes of the two orbits.

Halley suggested not only a sphere below the earth's outer crust, rotating with greater angular velocity than the surface above it, but also a second smaller sphere within the larger one with an angular velocity differing from each of those above it. Supposing our moon to be the interior of Perdita's similar spheres, and the asteroids to be fragments of the outer crust, then if the intermediate sphere contained the undiscovered mass which is necessary to supply the force of gravitation existing between the orbits of Mars and Jupiter (as stated in my letter of 29th March) that sphere must have been scattered in the form of meteoric stones, or

dust in one or more vast rings too attenuated to be easily seen, but forming an aggregate mass equal to a quarter or a third of the mass of Saturn; and to supply that material the total mass of Perdita at the time of the wreck must have been at least half as great as that of Saturn.

In that manner the transverse action of gravitation gives a clearly defined explanation of the important phenomena in the solar system which have recently attracted attention in consequence of the discovery of Jupiter's eighth satellite. The backward motion of that satellite in fact serves as an object lesson in illustration of that transverse action, and has incidentally brought from almost forgotten records a remarkable tribute to commemorate the genius of one of the most genuine among the ranks of scientific men—Edmund Halley.

15th April, 1909.

> 'The moon takes up the wondrous tale
> And nightly, to the listening earth,
> Repeats the story of her birth.'

I some years ago heard that Thomas Henry Huxley had on the previous evening spoken for an hour to a keenly attentive audience in discussing a small, shapeless fragment of chalk which he sometimes placed on the otherwise bare table before him and sometimes held in his hand to glance at for inspiration as he interpreted the story it had to tell. We all know, of course, that the chalk kept, and still keeps, a great deal to itself, but, nevertheless, the scientific world, I think, agreed in believing the story to have been truly told, so far as it went. The trouble with the moon is that there are elementary differences of opinion among us as to her meaning—that is to say, as to the character of the action by which she causes tide waves to flow over the surface of the earth.

In a paper on the Elements of Astronomy, which formed the basis of an address to the English Literary Society of Buenos Ayres on the 20th August, 1879, I treated as the best way to arrive at a clear understanding of the character of the earth's motion the supposition that she was once at rest on the surface of the sun, and then flung away by the action of the sun's rotation, like a drop of water from a twirling mop; the principle involved being the same if, instead of supposing the two bodies in contact, we start with all existing conditions.

An opinion as to whether the earth did, or did not, originate in that manner would be a mere guess, so far as any argument I could bring to bear on the question is concerned. But as regards the moon, Sir Robert Stawell Ball, together with Sir George Howard Darwin, have elicited from the tidal action resulting from the earth's rotation a marvellously romantic story, to the effect that a tide on the earth's surface whilst in a molten state was torn away by centrifugal action, and gradually increased its distance so as to become what is now the moon which revolves around us.

That story makes the relationship between the earth and moon truly that of mother and daughter; whereas my suggestion to the effect that our moon is in fact the Perdita which once revolved as a planet between the orbits of Mars and Jupiter makes them both daughters of the sun and sister planets.

The evidence I have given to the effect that the moon is in fact the lost planet of the solar system is, in my mind, the only directly valid argument against the idea of her having been evolved by tidal action from the earth, coupled with the natural inference to the effect that the earth was itself previously evolved in a similar manner from the sun. But Darwin and Ball consider that a long chain

of mathematical demonstration they have elaborated in connection with the moon's progress from the earth's surface to her present orbit places the relationship of mother and daughter beyond dispute.

We must not, however, in connection with this subject forget the well-known warning given by Huxley to the effect that the value of information obtained from mathematics depends on the validity of the basis on which any questions submitted to that oracle have been founded.

Darwin and Ball, in fact, asked mathematics to say what would be the effect of tidal friction, taking it for granted that the oceanic tides raised by the moon create such friction in opposition to the moon's action; and I do not doubt that the splendid array of mathematical demonstrations which have been published in 'The Philosophical Transactions of the Royal Society' and some other scientific journals correctly answer that question. But does such tidal friction as is assumed throughout the mathematics in fact exist?

If we accept as correct Dr. Whewell's chart of the Tide Waves of the World, which has been republished in Sir George Darwin's work on the Tides, then undoubtedly such friction must be supposed to exist; but a long array of tidal records gradually accumulated by the Admiralty have in fact shown the main feature of that chart to be absolutely at variance with the course of the waves which are actually flowing over the surface of the earth.

The mathematical enquiry has been based on the assumption that a wave follows the moon all round the Great Southern Ocean, as supposed by Dr. Whewell; but the Admiralty records which have accumulated since Dr. Whewell's chart was published have now made it clear that the assumed tide wave following the moon round the Great Southern Ocean does not in fact exist. There are

great tide waves all round that ocean, but they flow *eastwards* to meet the moon, not *westwards* following the moon. In a treatise on the Action of Vis Inertiæ in the Ocean, which I published more than forty years ago, I explained the action by which, as a tide moves westwards in the equatorial regions under the moon's direct action, the earth's gravitation maintains the equilibrium of the ocean on its surface by drawing an equal volume eastwards through the temperate zones; so that, as far as tidal action is concerned, there is just as much friction eastwards as westwards, as truly as, in both the Northern and the Southern Hemisphere, there is just as much tidal friction northwards as southwards. Rival theories have, however, long ago said their say, and the Admiralty Tide Tables are now the proper battle-ground on which to decide the question.

The forces of gravitation by which we are surrounded do in fact act as a constant drag against the earth's rotation; but the amount of that retarding action is neither increased nor diminished by the tidal action of the sun and moon. That question—as to whether the tide waves are such as to create more friction in one direction than another—is now one to be decided by recorded facts rather than by mathematics or by any abstruse theorising, and Perdita's story of the transverse forces of gravitation is fairly at issue with that based on the idea that the tide waves of the Great Southern Ocean follow the moon all round the earth.

17th April, 1909.

The Friday evening lecture on Experiments at High Temperatures and Pressures, delivered by Professor Richard Threlfall on the 19th March, was one which would, I am sure, have delighted so keen an appreciator of well-directed work as Edmund Halley, though it would have been difficult

for him to find his bearings among the tersely stated results of investigations carried far beyond what was the domain of science in his day.

As I listened to Professor Threlfall I drafted in my mind a letter for you ; but I am sorry to say that I waited before putting the letter on paper to see the lecture in print, and I find that though I have now before me perhaps more material in the form of skeleton, the flesh which gave life to the address delivered by Professor Threlfall is non-existent in the printed report, and I preface what I have to say with the foregoing only because it is connected with the subject of Professor Threlfall's experiments, and was called to mind by his address.

The subject of his lecture is also of interest in connection with the suggestion I have since made to the effect that our moon is the nucleus of a planet which once revolved in an orbit between those of Mars and Jupiter. As regards this latter point, I notice that in an essay on Sir Isaac Newton and Modern Chemistry which I published in the year 1900, I urged that ' in Nature's great forge, or crucible within the earth, and in the more stupendous forces of its greater crucible within the sun, natural transitions between matter and immaterial force may be effected.' I will not now attempt to repeat arguments which I have published from time to time duing the past forty-three years, but merely propound a question which suggests itself as to whether there is any evidence to be found of such a transition state between immaterial force and material substance having existed below the surface of Perdita when wrecked.

In the transit from her former to her present orbit she not only changed the radius of her orbit, but also the inclination of its plane to that of the sun's equator ; so that any light material flung away by a succession of volcanic actions might take very various orbits ; and the now

extinct volcanoes visible on the moon's surface may possibly have been the source of all the comets now wandering about the solar system.

Perdita's story, as told in my recent letters, seems thus to give a gleam of evidence in support of the argument of the above-mentioned essay that ' in the great crucible of the sun Nature is not only incessantly transmuting material substance into light, but is also just as incessantly creating matter by transmutation from some pre-existent form of force.'

I postpone the original point of interest in connection with Professor Threlfall's lecture.

26th April, 1909.

At our Friday evening lecture on 20th May, 1904, Professor Ernest Rutherford gave us a very interesting account of investigations in connection with radio-activity which he had conducted jointly with Professor Frederick Soddy, and a book just published, on ' The Interpretation of Radium,' by Professor Soddy, deals with a further most interesting development of experimental demonstrations and of views based on them by leading experts.

As an important point in the views I have published during the past forty-three years is to the effect that we are quite unconscious of the real amount of the pressure of force in which we exist, and are only now beginning to discover its existence by the use of our intellectual power, it seems opportune to consider how that view stands in presence of the progress of scientific knowledge recorded in the above-mentioned work.

The scientific merits of the book are not disparaged by its omission of what I cannot help considering an important point in the history of scientific opinion. Professor Soddy

asks: 'Is there anything opposed either to reason or to probability in the view that the energy evolved from radium is actually derived from an existing previously unsuspected internal store within the atom? How is it that such enormous stores of energy in matter have remained so long unknown?' The fact is, however, that throughout my writings atomic force has been considered to be quite great enough to be equal to any demands the revelations of radium may make on it. When two particles of matter are in contact, then the amount of the reciprocal action of gravitation which can become effective between them depends on their shapes, and may be infinitely greater in one case than in another (see Essay on Newton and Modern Chemistry, pp. 154–5). Particles of matter in thorough contact—that is to say, not merely touching each other at some points, but in such contact as to leave no space between them—I do not suppose to exist in nature except in the elementary atoms of chemistry; and if we suppose the universe to be infinite in extent, then we must also recognise that the force with which the particles which constitute each one of those atoms are held together is infinite. I do not suppose the universe to be infinite, but so great that we may for all practical purposes regard the force with which an atom holds itself together as practically infinite. The gravitation of the whole universe is acting reciprocally on each particle of the atom, so that two forces each equally approaching to an infinity of power are incessantly conflicting in tension in each atom. Thus, as far as quantity of force is concerned, each individual atom contains all that can be called for. Any action in connection with the atom is due to relatively slight differences in the action of infinite forces; and those *relatively* slight quantities of force may be absolutely very great; or, in other words, that quantity of force may be enormous in relation to the forces with

which we have practically to deal in ordinary terrestrial physics.

In further support of the idea of the energy of radium being contained within it, Professor Soddy records facts which he considers make 'the alternative, that the energy of radium is derived from outside, well nigh incredible. For to account for the energy evolved from the emanation we must suppose all space to be everywhere traversed by new and mysterious forms of radiant energy of such tremendous and incredible power that the explanation is harder to believe than the fact it is supposed to explain.'

The infinite force by which the particles which compose the atom are held together is, however, a consequence of the reciprocal action between each of those constituent particles and every other particle of the universe; and the internal and external forces are, therefore, merely opposite aspects of a force of gravitation exerted by the whole universe. They are concomitant forces, and neither could exist independently of the other. The pressure created by gravitation within the atom and the tension created by gravitation outside it, as lines of force connecting it with every other atom of the universe, supply as ample a store of force as any theory can call for either inside or outside the atom.

In the treatise on Vis Inertiæ which I published in the year 1868 (forty-one years ago) I argued that 'the force which causes motion is being constantly spent by the resistance of *vis inertiæ*, and the creation of gravitation is as constant—for it is in fact the same act—as the destruction of the force which causes motion. It is in constant process of creation from a pre-existing force which is not gravitation, and is as constantly being transmuted into some other force or forces, which, be they what else they may, are not gravitation either' (p. 211).

Some of the startling phenomena recorded by Rutherford and Soddy bring those views back to mind, together with the argument given in 1866 in 'The Elements': 'There cannot be absolute and constant equilibrium in any part of the universe. For it is a transitory phase between the unknown which has been and the unknown which shall be' (p. 75).

'Matter is a connecting link between forces which have passed, or are now passing away, by transmutation into matter, and other forces which have been created, and continue to be incessantly in process of creation, through the evanescence of matter' (*B.A. Herald,* September 22nd, 1881).

I am, dear Sir,

Yours truly,

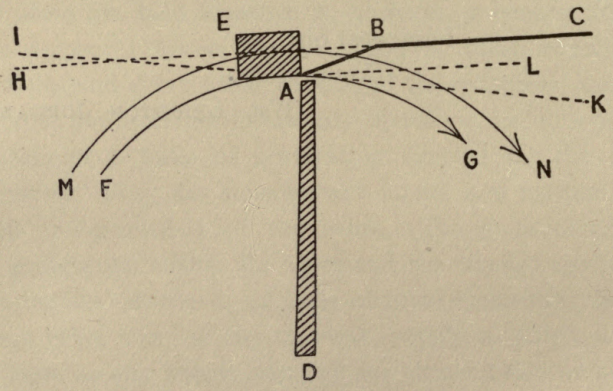
WM. LEIGHTON JORDAN.

Letters to the Institution of Civil Engineers

NEWTON AND THE RAILWAY TRUCK

27th January, 1909.

In discussion of the papers on railway curves: Let D A in the annexed diagram be an iron bar arranged to revolve round the centre D.



E A—a piece of iron attached by string to the bar at A so that whilst the bar revolves the point A of the mass A E marks on the floor the circle F A G, which therefore touches the inner edge of the revolving body.

M N—the circle along which the centre of gravity of the revolving body travels.

I K—tangent to the circle described by A at the moment of the breaking of the string.

H C—tangent to the position of the centre of gravity of E A at the moment of the breaking of the string.

A B C represents the line described by the point A on the breaking of the string at A in each of three experiments which I made some time ago.

In each of those experiments the line A B deviated about 30 degrees from the tangent I K; and the line B C was clearly a prolongation of the tangent H B passing through the centre of gravity of E A. The angle A B C will doubtless vary with change of shape of E A and its velocity.

It is evident that if the centre of gravity of E A follows the tangent H C, and the point A moves into that tangent instead of following its own tangent I K or the line A L parallel to H C; then the point E must at the same time also move into the same tangent, H C, but from the opposite direction. And therefore whilst the mass E A is being revolved round the curve F A G it must be subject to a constant effort to rotate on its own centre of gravity in the opposite direction to that of its motion of revolution along the curve.

The above experiment tends to show that every railway engine, carriage, or truck, whilst rounding a curve, has a tendency to rotate on itself in the opposite direction.

Mr. Jacomb-Hood told us that in experiments with one of his engines, it would, when travelling at a high velocity, go safely round a given curve, but would invariably climb the outer rail and leave the track if travelling slowly; and considered that result to stand unexplained.

I venture to suggest that the probable explanation is that at the high velocity the rear outer wheel, as well as the leading outer wheel, is kept well pressed against the outer rail; but with the slower velocity the rear inner wheel is able to reach the inner rail, and its action in combination with that of the leading outer wheel causes the latter to climb the outer rail under the tendency of the engine to rotate on itself.

A remarkable point in Mr. Spiller's paper, and also in Mr. Shortt's contribution to its discussion, is the argument to the effect that the truck whilst travelling round the curve is at the same time rotating on itself in the same direction; the difference between them being that the former makes the rear outer wheel the centre of that rotation, and the latter places the centre of rotation at the centre of the rear axle.

I think I am safe in saying that the idea of a railway truck being subject to any such motion of rotation whilst rounding a curve has never before been suggested in the Proceedings of our Institution.

You have, however, in our library a work entitled, 'The New Principles of Natural Philosophy,' in which I have, I believe, shown that if the arguments which Newton bases on his definition of *inertia* in his astronomical work are valid, then in all such phenomena as that now under discussion we must suppose the existence of a motion of axial rotation at the same time as the motion of revolution round the curve, and in the direction asserted by Mr. Spiller and by Mr. Shortt.

That rotation is, however, a purely theoretical idea, necessitated by Newton's definition of *inertia*, or (as Mr. Shortt put it in his argument) by the First Law of Motion, and most modern scientists who continue to accept Newton's argument on the subject deny that any practical action of force is connected with the phenomena.

Neither Newton nor any of his contemporaries ever applied (so far as I know) their astronomical argument on this point to any such terrestrial object as a railway truck.

In Mr. Spiller's paper the argument is all based on the diagram of a truck given in *plan*. Let us, however, suppose that diagram to represent the truck in longitudinal section

instead of in plan. Then, as regards the point in discussion, the same argument is applicable to the motion of the truck round the curve of the earth's surface as that applied to its motion round the railway curve.

The fact is that the definition of *inertia* which Newton adopted from his predecessors served its purpose admirably for advancing explanations of astronomical phenomena; but I assert that no practical engineer can possibly apply Newton's astronomical argument logically to the motion of the railway truck, either round the curve of the earth's surface or round the railway curve, in such a manner as to decide when, with continuity of revolving motion, a motion of rotation on itself would be completed; and in the absence of such a demonstration the application of Newton's astronomical argument to terrestrial phenomena involves its *reductio ad absurdum*.

The reason for what my experiments indicate to be an existing and very practical effort of the truck to rotate in the opposite direction to that in which Mr. Spiller and Mr. Shortt suppose it to rotate is, I consider (as also in my experiments), the relative amounts of resistance to the revolving motion. The resistance to the motion is as the square of its velocity; and is, therefore, greater on the part over the outer than on that over the inner rail, creating therefore a tendency for the outer side to be drawn back behind the centre of gravity, and at the same time giving the inner side a tendency to spring forward in advance of the centre of gravity.

Note.—Mr. Spiller (his page 7) says that the angle through which the truck turns, in the motion of rotation round an axis within itself, on any curve is equal to the angle of incidence between the tangents to the curve at its extremities. And, though he does not in his Paper assert that to be in accordance with Newton's idea of axial

rotation, he tacitly acquiesced in Mr. Shortt's assertion to that effect. That idea may be justified by somewhat vague general arguments to be found in some modern text-books, but not by any statement to be found in Newton's works. To make the axial rotation of the truck on itself accord with Newton, the moment at which the truck enters the transition curve from the tangent and the bearing of the tangent at that moment in relation to the 'fixed stars' must be ascertained in order to have a basis for Newton's argument; and the angle through which the truck turns on itself in relation to that line (not that in relation to the changing position of the tangent) would give the axial rotation according to Newton's theory.

I beg, however, to be allowed to remind the scientific world through the published Proceedings of the Institution of Civil Engineers, that when Galileo invented the idea of *inertia* on which Newton based his First Law of Motion, the stars were supposed to be really fixed in space and therefore to give a firm basis for the measuring of angular motions in astronomical phenomena; but the discovery of the fact that the stars are 'wanderers' as truly as ourselves and our sister planets insidiously removed the foundation of the theory of *inertia* invented by Galileo and adopted by Newton.

THE QUEBEC BRIDGE

10th February, 1909.

In the discussion on Cantilever Bridges on 11th February[1] of last year, I spoke at some length in criticism of Professor T. C. Fidler's Paper, concluding to the effect that it appeared to me incredible that the formula given had been used in actually building any cantilever. And on 12th May,[2]

[1] See pp. 103 to 106. [2] See pp. 99 to 103.

in a contribution to the correspondence on the King Edward VII. Bridge over the Tyne, I described a stress in the anchor arm of the Quebec Bridge which appeared according to my argument of 11th February, 1908, to have amounted to about 19,000 tons at the moment the bridge fell instead of the generally estimated amount of 8,000 tons.

The object I now have in addressing you is to record justification which has since become available for both the above-mentioned contributions.

I have not had an opportunity for reading the Report prepared by Mr. C. C. Schneider for the Canadian Government; but I find that the Editor of 'The Engineer,' in a leading article on 28th August last, says Mr. Schneider considers that in such large structures 'the lines of stress should be as direct as possible. The compression or lower chord should not be arched upwards but straight, so that it might carry the thrust directly to the pier.'

A glance at the Diagram and Table of Stresses, given in my Part II. of 'The Sling,' pages 98 and 101 respectively (or my letters of 15th April and 12th May to you), shows that if the anchor arm of the Quebec Bridge had been constructed in that manner, then the stress of about 19,000 tons indicated by my Table of Stresses would have been reduced to about 11,000 tons; and all I claim for the method I have given is that it is an easy way of arriving at a sufficiently close approximation of stresses as to prevent such a mistake as occurred in the construction of that bridge.

I do not see the reason for applying Mr. Schneider's straight thrust between the main piers of the bridge; nor is it clear to me that it was wanted in the anchor arm except during construction; as the reciprocal support given by the meeting of the cantilevers over the river must cause (or might be so arranged as to cause) a great difference in the stresses in the anchor arm.

It is now evident that the leverage stress which I described in connection with the above-mentioned discussions was overlooked in the construction of the Quebec Bridge; and I frankly say I am inclined to think that a scare caused by the disaster among professional engineers is now causing them to be hypercritical in condemning what was in other respects an admirable design, which was first scanted for financial reasons, and then cruelly overtaxed by that great oversight. The consolation to be gleaned in the disaster is the proof it has made apparent of the reliance which may be placed on at least one firm of iron founders for honest and splendid work.

Professor Fidler, in the *Times* of 18th March last, says there are two impressions caused by the fall of the Quebec Bridge which seem likely to survive. First, that the fall of the bridge cannot be explained by the ordinary principles of bridge theory, and, secondly, that the professional knowledge of the present day is in some way at fault.

Professor Fidler then proceeds to defend what he alludes to as present-day ordinary principles by argument which does not remove either of those two impressions from my mind, as it reads to me simply to the effect that the bridge ought to have been so constructed as to stand the stresses we now know to have been brought upon it; but that is beside the question, for the position is that, as that great leverage stress in the anchor arm, wrenching the anchorage bolts upwards through the shore pier and at the same time crushing the shoes on the main pier and driving them shorewards, was not recognised, it was naturally not provided for. The special point overlooked seems to have been the vertical *upward* component of the thrust from the main pier to the anchorage pier, shown in the Diagrams of my book above alluded to.

A circumstance which tends to justify the attitude of my mind in the foregoing question is the present difficulty with the Queensborough Bridge over the East River between Blackwell Island and New York—a larger and heavier cantilever than the Quebec Bridge and recently supposed to be almost ready to be opened for traffic.

The disaster at Quebec led to the appointment of a Commission to report on the New York Bridge; and, according to 'The Engineer' of 4th December last, they have reported it 'to be defective in the same manner as the ill-fated Quebec Bridge.' It is said that though designed to carry a load of 16,000 pounds per lineal foot, it cannot safely be trusted to carry more than 6,000. Such great miscalculations of the stresses in two immense bridges building at the same time seem to indicate that 'present-day ordinary principles' have omitted an important stress in both of those constructions.

THE STRESS OF WATER PRESSURE

27th February, 1909.

In letters dated 30th January, 8th March, and 12th March of last year I addressed you in connection with the discussion of the Papers on Water-Pressure in Masonry Dams which were read on the 21st January of that year.

The two main points of those letters are: First, the specification of an important error as regards hydraulic action in the Paper presented by Sir John Otley and Dr. Brightmore; and, secondly, the substitution of a new theory of Hydraulic Action in Masonry Dams for that which Sir John Otley and Dr. Brightmore have based on the error above alluded to.

Those letters have not been directly noticed in the

published Proceedings of our Institution, and I am led to understand that the reason for my contribution having been so slighted is because the Council consider my imputation of an error in the Otley-Brightmore Paper to be due to want of correct knowledge of hydraulic action on my part, and that no such error as asserted by me does in fact exist in that Paper; and it has therefore been published in our Proceedings as read at the meeting, ignoring my statement to the effect that it is vitiated by a fundamental error.

My first object now is to make it quite clear that what I declared to be an error is in fact an error which requires the correction which I applied to it. I then considered it sufficient to say that if the authors would reconsider their argument on page 8 of their Paper (page 94, Vol. CLXXII. of our Proceedings) they would at once recognise that their conclusions had been arrived at by treating the water-pressure as varying as the *square of the depth* instead of merely as the depth.

That they have treated the water-pressure against the dam at different depths as varying as the square of the depth it would be worse than ridiculous for any reader to deny. And also every hydraulic engineer knows perfectly well that in two dams of different depth the *aggregate* of the water-pressure is greater in the higher dam in the ratio of the *square of the depth*, but that in each of the dams the water-pressure at different depths varies *equally* with the depth, not as the square of the depth. The latter idea could only be entertained through one of those strange moods of thoughtlessness into which the most able and learned may sometimes lapse.

Not only was the error properly corrected by me, but also it was essential for me to correct it in order to bring forward my new views; and it is not possible for any

hydraulic engineer who gives the matter his consideration to do otherwise than recognise that on the above point the Otley-Brightmore Paper is mistaken.

Now as regards the new theory of stress which I submitted to the Council: Sir John Otley and Dr. Brightmore in their Paper considered it 'desirable to apply mathematics in detail, rather than to attempt to make one complete mathematical problem of the whole question.' The attempt which they deliberately refrained from making is, in fact, made in the contribution which I submitted to you for the Correspondence Discussion.

The invention of a formula for dealing in the aggregate with the stress created by water-pressure in masonry dams was in fact the object of my contribution; and my exposition of the error in the Otley-Brightmore Paper was merely incidental in that purpose.

They say that if the width of a dam increases as the depth, the mean intensity of the shearing stress on each horizontal plane must decrease as the height of the plane above the base increases because the water-pressure increases as the square of the depth. That is the point which I challenged.

In the first instance I gave reason for considering the stress at the surface to equal that at the bottom of the dam, even without allowing for the increase relatively to the decreased thickness of the dam at the higher level (which in the dam under consideration is inversely as the height).

I then took it for granted that the stress at the top being the same as at the bottom, it would be the same all the way up; but the formula which I afterwards formed and submitted to you shows that though the stress against the top half is equal to that against the bottom half of the dam, it is not equably distributed all the way up, but that

nearly half the stress is given in the middle third of the height and not much more than a quarter in each of the other thirds.

The formula as I gave it to you is very simple and, as far as I know, it constitutes the first indication for dealing with the stresses created by water-pressure by a single calculation which gives at the same time the aggregate stress and the detail of its subdivision at all heights.

I believe the value of the unit of stress to be $31\frac{1}{4}$ lb. per square foot, like the unit of pressure as given in my illustration of the Assuan Dam ; but that is a point which requires some further consideration, as I have there said.[1]

Whether that value is accurate or not, I beg to be allowed to urge on the Council that it would have been well to allow the formula to be recorded in our Proceedings as a tentative step in a question of vast importance. If not found correct as it stands, some simple and easy modification would certainly be found to make it so. What is clear is that in dams of different height the aggregate of the water-pressure against each foot length is as the square of the depth of water, and also that the stress created by that pressure is as the cube of the depth ; but the precise ratio which the unit of stress bears to the unit of pressure I have not determined to my satisfaction. If it is less than a ratio of equality, then the ratio which stress bears to pressure must increase as the depth increases.

17th March, 1909.

In the discussion of 9th and 16th instant on Mr. Wade's paper on Dam Construction in New South Wales some startling statements were made which embolden me to place before you a record of several occurrences which

[1] See page 197, making the unit of stress $\frac{2}{3}$ of $31\frac{1}{4} = 20\frac{5}{8}$.

when read in juxtaposition, throw a glaring light on the present position of theory as regards the action of water-pressure in masonry and other dams.

First.—Sir Benjamin Baker, speaking on the 28th March, 1905, in our Lecture Hall, said that during the previous twelve years perhaps half a dozen dams had been washed away; but he could not mark any great advance in the theory of dam construction.

Secondly.—A labouring man states that on the 6th of this month, whilst he was standing on the wall of a new dock in course of construction at Birkenhead, where fifteen of his comrades were working at the foot of a dam 100 feet long and 40 feet high, suddenly 'the bottom seemed to flit away from where the men were working, while the top of the dam closed over them like the lid of a box' (*The Observer*, 7th instant).

That accident seems to justify Sir Benjamin Baker's warning as to the expediency of allowing ample margin for safety beyond the strength indicated by generally accepted theory.

Notwithstanding that position, whilst fourteen unfortunates lay entombed through some misunderstanding of the stress to which that dam was subjected, opinions were freely expressed at our meetings above mentioned to the effect that the success of the dams in New South Wales, described in the paper then read, showed that our engineers are now generally wasting material by giving an unnecessary amount of strength to dams.

The Birkenhead accident seems to combine with Sir Benjamin Baker's warning to make any deliberate reduction of strength in dam construction unjustifiable at present.

It is appalling to think that so soon after Sir Benjamin Baker's warning as to the imperfection of our knowledge of the effect of pressure in dams, several of our leading

authorities, including perhaps the highest practical authority in dam construction, should declare in effect that we are indebted to the intelligent courage of Australian engineers for having shown that we have been over-cautious and have wasted material by requiring a superfluity of strength in our dams.

I venture, most deferentially, to say that a perusal of the Paper under discussion does not appear to justify the charge of over-caution which one of the engineers alluded to admitted as valid against himself and other old-world engineers.

I understand that, as a matter of fact, all the comparatively narrow walls in Australia are those of dams which are horizontally arched ; and, if so, no engineer can for a moment venture to contend that the same width and strength of wall carried across a valley in a curve with its convex side presented to the water-pressure, as in the Australian dams, ought to suffice for a dam built straight across the same valley, and therefore, even if the curved dam is proved to be the more economical form for resistance, that throws no new light on the theory of water-pressure.

Australian engineers may perhaps have shown that curved dams, notwithstanding their greater length, may be constructed of sufficient strength more economically than straight dams, but that does not affect the evidence given by the disaster at Birkenhead, for in that case it was necessary to use a straight dam, and it clearly justifies Sir Benjamin Baker's warning to the effect of the existence of a tendency to under-estimate rather than to over-estimate the force of water-pressure, and as to the absence of any reliable theory of the action of water pressure against a dam.

I was much interested in Colonel Pennycuick's modification on 16th instant of what he said on 9th instant, and especially in his suggestion to the effect that a relative width

to height found to be sufficient in a low dam may not perhaps be sufficient in dams of greater height. That opinion, expressed by so high an authority, if correct, makes the unit of stress alluded to in my letter of 27th February less than that of pressure, as there suggested. It seems to me at present probable that the unit of stress is two-thirds of the unit of pressure, but I do not see any theoretical proof.

I have to thank you for your letter of 11th instant, though it compels me to reiterate what I have on a former occasion urged, to the effect that I most thoroughly recognise that the question as to whether any contribution offered to you is or is not to be published in the Institution Proceedings must, of course, depend entirely on the judgment and discretion of the Council. You know, however, that the judgment and discretion of our Council, like that of every Council that ever was composed of human beings, is liable to go wrong ; and the object of my letter of 27th February is to urge upon them that the complete omission of my contribution to the Otley-Brightmore discussion was ill-judged and indiscreet, and also to pave the way for a rectification of that mistake in connection with the discussion of Mr. Wade's paper on Dam Construction in New South Wales, which had then been announced. I sincerely hope they may see their way to take that course, not merely as fair to me personally, but as called for in the general interest of the Institution.

You tell me that it is open to all to participate in the discussions at the Institution ; but I ask you to remember that what I spoke in the Hooghly River discussion on the 17th January, 1905, was ignored in our published Proceedings ; and it, therefore, suits me better to contribute in writing so as to have the contribution readily available for publication

elsewhere if rejected by our Council. The error they made in recording the Hooghly River discussion looms larger and larger as time flows on.

In the latter part of the year 1856 Joseph Garibaldi expatiated to me on the difficulty of navigating the Paraná off the shore of Entre Rios in consequence of the incessant changing of the channels. We were standing on the stern of a small schooner which he was guiding by scanning the swirls of the river stream. He was at first surprised, but soon keenly interested when I took an orange from the deck and explained to him that the earth's motion gave the river a tendency to press against the Entre Riano shore subject to alternations in the amount of that action not only throughout the year but also throughout the day. Garibaldi was captain of the craft and owner of its cargo of oranges, perhaps also of the craft itself; and after we had anchored for the night I had the pleasure of overhearing from the cabin where I was sitting with a fellow-passenger a lucid explanation given by him to his crew, whose questions and comments clearly showed them to be as keenly interested as Garibaldi's replies showed him to have grasped the question and to be an able exponent of it. I was then coming down the river from Paraguay to Buenos Ayres, and had recognised the effect of the earth's rotation on it during the upward voyage as according with Hadley's theory of the Trade Winds.

I beg the Council of our Institution to consider the attitude of those five or six Italian sailors towards the question, and then reflect on their own treatment of my contribution to the Hooghly River discussion in our Lecture Hall fifty years afterwards.

20th April, 1909.

I hope I am not too late with this contribution to the Correspondence Discussion of Mr. Mallock's Paper on

Construction and Wear of Roads. I obtained a copy when it was in course of discussion on 6th instant, intending to write immediately, but have had my attention distracted from the subject.

You abstained from publishing in our Proceedings the statement I made in the Hooghly River discussion to the effect that the tide waves of the Great Southern Ocean flow *eastward*, to meet the moon, not *westward*, following the moon ; but until it is recognised that such is the case, and until the theoretical action which makes such the case is clearly understood, it is impossible properly to understand one of the most important principles involved in the wear and tear of roads ; and a proper understanding of that principle is essential for arriving at the best form for construction.

Turning to Mr. Mallock's Figure 5, and taking the wheel there depicted to be the leading wheel of a cyclist's bicycle, it may, for all practical purposes, be considered that the first effect produced on any given part of the road traversed is at the point where the wheel begins to tread the ground as it advances. Theoretical action, which may for practical purposes be ignored in connection with that bicycle, becomes, however, of vast practical importance in connection with heavier and faster traffic. In illustration let us take a steamer of 30,000 tons crossing the ocean at a speed of twenty knots an hour. In that case the first effect on the pathway traversed by the vessel is not where her prow treads the water, but a long way in advance of that position, perhaps often more than a ship's length in advance ; and that first effect is not a piling up of the water by the pressure of the ship's motion, but a *sinking of the ocean level* to form a tidal wave with its crest astern of the ship, so as to preserve the balance of gravitation disturbed by the ship's motion.

If, instead of moving through the water, we suppose that

mass moving on land absolutely rigid all the way to the centre of the earth, then its motion must be incessantly changing the position of the earth's centre of gravity; that is, however, a state of affairs which does not exist in practice, and the work corresponding to that done in creating the tidal wave which accompanies the steamer is on the land expended in vibrating the roadway and surrounding ground.

The fact that such reaction against the motion of a railway train, or a carriage on a roadway, must be as the weight multiplied by the square of the velocity (under otherwise similar conditions) shows the necessity of both a weight limit and a speed limit if the cost of maintenance of a roadway is worth consideration. If the average weight of vehicles is doubled and also the average speed (other conditions being the same), then eight times more force than before is exerted to tear the road to pieces. The increase of weight may, of course, be much more pernicious under some circumstances than indicated by that rule, as a road able to carry an incessant traffic in vehicles under a given weight might be destroyed by the passage of a single vehicle above that weight.

Westminster Bridge has in recent years become an interesting object-lesson in road stresses in consequence of having been obliged to carry much heavier weights than contemplated when the bridge was built. I some years ago tested the stresses along both sides of the Bridge, and was quite startled by the distinctness of the thrusts of some of the heavy traffic far away from the position of the vehicle—the builder of the bridge would have been broken-hearted if he had seen it subjected to such treatment. As a vehicle ascends the slope of the bridge the increase of stress due to the slope is of course equal to the increase of force requisite to draw it up the slope instead of along a level; but the most pernicious thrust is not given locally, but in the corresponding position

at the opposite end of the bridge, making practically apparent the immense increase of stress due to working uphill on any road, and the special importance of making the roadway over any bridge level, even at the cost of some inconvenience in arranging necessary inclines at the approaches to the bridge.

In a discussion on the Manufacture, Use, and Wear of Rails, on 10th April, 1900, Sir Lowthian Bell gave a clear explanation of the manner in which a fast train may run in safety over a fractured rail which could not carry the same train if moving slowly ; and I added a rider which appears to me to be even more important for those responsible for the practical working of a railway line to bear in mind, to the effect that the vibration caused by a fast train might break rails, or shake to pieces a girder, which would not be injured by the passage of the same train moving slowly. The latter is a maxim of incessant importance on road or rail, whereas the other a driver may seldom have occasion to act on. Improved construction of roads is, of course, a matter of vast importance in the general interests of the country ; but, whatever the road may be, the question of speed is one to be determined by those who have to pay the necessary cost of maintenance for high speed. A crowded road of light vehicles running in both directions must cause less vibration and disturbance in the neighbourhood of a roadway than the same amount of traffic carried in a few heavy vehicles, because the vibrations must to a greater extent annul each other by their counteractions.

The ' Mauretania's ' mass of 30,000 tons moves smoothly because the roadway she traverses is so elastic that it automatically restores the equilibrium of gravitation as fast as it is disturbed by her motion, but on land the amount of elasticity which would make the easiest and best road for light traffic must, for that very reason, be unable to carry

heavy traffic ; and the amount of elasticity given to a road must therefore be limited by the amount of weight any single vehicle is allowed to carry.

We must not either lose sight of the fact that a large amount of the country's wealth has been invested to provide railways for fast and heavy traffic, and the expenditure of ratepayers' money to assist such traffic to tear the public roads to pieces, so as to escape reasonable charges involved by using existing railways, is a course to be jealously kept within bounds in the general interests of the country.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

PART IV

LETTERS IN CONNECTION WITH THE ROYAL INSTITUTION THE INSTITUTION OF CIVIL ENGINEERS AND THE ADMIRALTY

APRIL to NOVEMBER 1909

THIS VOLUME IS DEDICATED

TO

SWINFEN KER JORDAN, ESQ.

OF

BUENOS AYRES

IN RECOGNITION OF HIS KIND APPRECIATION

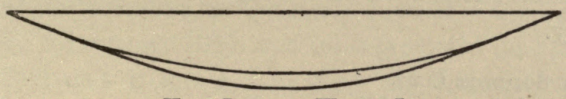
OF PREVIOUS PARTS

PREFACE TO PART IV

I HAVE reproduced from Part II. the annexed very simple diagram showing two different shipshapes because it shows the main feature in the transition from what may now frankly be termed the exploded principles of Sir George Stokes to the new principles which are now practically adopted by Naval Constructors in all countries.

An eminent authority writing in the *Pall Mall Gazette* on 4th instant (September, 1909) tells us that in the German

SHIPSHAPES



HALF-PLAN AT WATER LINE

Diagram to illustrate the principle involved in the transition from the 'King Edward VII.' to the 'Dreadnought' type of battleship, which constitutes a practical abandonment of the idea of *inertia* originated by Galileo and adopted by Newton. The principle is the same if the width of beam is altered at the bow and stern instead of amidships, or all along.

Navy the battleships now building differ from their predecessors by an increase of beam and reduction of draught; and I have seen it stated that the 'Minas Geraes' just built in this country for the Brazilian Government will draw only $25\frac{3}{4}$ feet when displacing 22,000 tons, whereas our 'King Edward VII.' draws more than 28 feet when displacing less than 18,000 tons.

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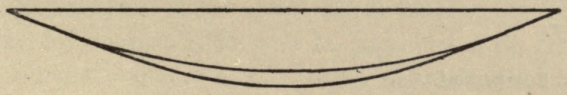

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The series of letters commencing 12th January, 1903, which I addressed to the Institution of Civil Engineers and afterwards to the Admiralty (the latter now published in the Appendix to this volume, and some of the former already published in Part II.) gives, to the best of my knowledge and belief, the only exposition of the mistake that was being made by our Naval Constructors and some others at the time I commenced that series of letters. It was the purely theoretical considerations which I explained in those letters which led to the practical change which I have just above mentioned, and the reason I have for urging attention to the subject is because that practical change of shape cannot fail to be recognised before long as constituting a practical demonstration to the effect that what has long been termed the First Law of Motion is not true to Nature, and that the action of astral gravitation (which is incompatible with that so-called law) is a fundamental principle in all terrestrial phenomena resisting all motion with a force as the square of velocity.

ROYAL SOCIETIES CLUB,
24th September, 1909.

LETTERS

To Sir William Crookes, F.R.S., Secretary, the Royal Institution

28th June, 1909.

Dear Sir,—By an incidental allusion to earthquake observations made by Professor Emerson Reynolds in his Friday evening lecture of 28th May, I was reminded of Halley's suggestion (alluded to in my letters of 5th and 13th April) to the effect that the earth's rotation is not uniform all the way to the centre, but is subject to several different angular velocities.

Professor Reynolds referred to a Paper by Mr. R. D. Oldham published in the Quarterly Journal of the Geological Society for August 1906, and he reproduced on the screen Mr. Oldham's Diagram 2 to show the different velocities with which earthquake waves are propagated in different directions from the same source.

It seems to be a well-established fact that the propagation of earthquake waves is, in some manner, interfered with in the central parts of the earth, and, in the absence of any direct evidence to the contrary, that diagram may fairly be considered to favour the views of Edmund Halley.

I, however, suggest that explanation of the phenomena merely because it gives further evidence beyond what I have already recorded as indicating that our moon is really what has long been the ' Perdita ' Planet of the solar system, but such a vast amount of work has been done by Professor John Milne, as well as by Mr. Oldham, in connection with these

earthquake waves, that it must rest with them to decide as to whether their observations can be considered as confirmatory of the views of Dr. Halley.

30th June, 1909.

I am sure that on Friday evening the 14th February of last year I was not the only member of our Institution who felt thankful to you for having arranged to allow us the pleasure of hearing the lecture delivered by Dr. Saleeby on National Eugenics. I have since read it as Chapter XV. of his recently published work 'Race Culture'; and I make no excuse for addressing the Royal Institution on the part of Chapter IX., where the author says: 'Morality is not an invention of man, but man the child of morality.'

In any widely extended battlefield the ablest combatant may not be able to appreciate the importance of his point of conflict in relation to other parts of the field; and I am not sure that Dr. Saleeby is fully conscious that, in the argument from which I have extracted the above words, he is striving to gain a position which dominates the whole field of conflict. I can imagine no question now at issue which is more important, or even so important, in the highest interests of science and civilisation as that which he is unflinchingly maintaining in the above extract. The battlefield may be surveyed from many different points of view, and to reach one which differs somewhat from those generally held, I ask you to imagine yourself sixty-two or sixty-three years ago (1846 or 1847) leaving the Gloria Hill on the west side of the Bay of Rio in a boat for Piedade at the foot of the Organ Mountains. For part of the sailing, or rowing, you take the same course as was taken by Charles Darwin in the year 1832, when he made his memorable visit to the virgin forests of Brazil. That same

route was taken, I believe, by Dr. Joseph Dalton Hooker in June, 1843, when he visited Brazil with Captain James Ross with H.M.SS. 'Erebus' and 'Terror' returning from the Antarctic regions. They landed on the east side of the great bay, but you go to the north end, traverse an alluvial plain to Frechal and then ascend by a rough bridle path the romantic Organ Mountains, which they admired from the distance. On reaching the plateau three thousand feet above the bay, you may see far away on your right the famous fazenda house owning all the surrounding country, then pass a few scattered houses on the left and turn to the right over a bridge of sturdy tree-trunks. A narrow path leads through dense forest until you reach a clearing on the mountain-side covered with fresh green grass and scattered boulders of the class made famous long afterwards by Agassiz. At the upper end of the cleared space is the Araras cottage, and over the mountain behind it there extends the vast forest visited by Darwin and Hooker. Who cares to know of the exuberant plant and animal life surrounding that spot may learn from Darwin's pen. In the midst of it all two business men, day after day, ransacking the forest for their holiday amusement, carried on an earnest discussion which worried my wits more than either of them ever knew. The names of St. Hilaire and Cuvier impressed themselves upon me—the saint appearing to me sometimes strangely on the wrong side in the fray. 'But J., just think of those fossils you have at the Gloria; the mountains did not exist when the rocks were formed, and the fossils were living creatures before the rocks were created,' argued the young man. 'No, F——, I cannot admit any of that argument: it is just as easy for the Almighty to have created things as we now see them as to have brought them through all the stages you suppose,' was the calm reply of the older man. Each frankly argued from his point of view in unruffled terms; but my wits were

really saved from being lost in the matter by the merry banter which each disputant received from his charming wife when discussing indoors. One merrily declared that 'the Bible could not possibly have been intended to mean anything of that sort.' And the other: 'But, Alec, it cannot matter so much which way we believe it came about; it might have happened either way.'

Robert Chambers had before that time published anonymously 'The Vestiges of Creation,' and immediately on reading that work many years afterwards, I recognised that the elder of the two disputants was defending the views of Cuvier on the fixity of species, then triumphant in science, against the idea of evolution by development from antecedent forms, which the younger man must to some extent have advocated in support of the views, or some of the views, of St. Hilaire.

I now find, in one of the Cambridge Essays written for the Darwin Centenary, Dr. Bury sums up Darwin's position by saying: 'In the course of the dozen years which elapsed between "The Origin of Species" and "The Descent of Man" (1871), the hypothesis of Lamarck that man is the co-descendant with other species of some lower extinct form was admitted to have been raised to the rank of an established fact by most thinkers whose brains were not working under the constraint of theological authority.'

That may be taken as all very well so far as it goes; what it amounts to is that what St. Hilaire in the year 1830 failed to achieve against the opposition of Cuvier (then the giant of the scientific world), Darwin and Wallace with the support and guidance of Lyell and Hooker effected more than twenty years afterwards; but the Essays really go far beyond that in extending the influence of Darwin's work. Professor Höffding of Copenhagen says: 'It is a condition of individual and social progress that a man's mode of action should be

determined by his own character and not by tradition and custom, nor by abstract rules. This view was to be corroborated by the theory of Darwin.' And Professor Bouglé of Paris says: 'When objective sociology declares that even when social phenomena are in question, all finalist preconceptions must be distrusted if a science is to be constructed, it is to Darwin that its thanks are due.'

A great deal even more pronounced to the same effect in the Cambridge publication seems to place Darwin in the ranks of those against whom Dr. Saleeby is waging war. If that is so, then there is certainly a vast step from St. Hilaire to Darwin, but I do not remember reading anything of Darwin's to justify his being placed in those ranks; and for the present I will continue to believe that Dr. Saleeby, who is a broad-minded evolutionist in the organic as well as in the inorganic world, more truly than his opponents represents the ideas of Charles Darwin.

Each writer of the Cambridge Essays probably knows just as well as you and I do, Sir William Crookes, that the law of gravitation exists quite independently of us: we were not consulted in the making of it; but there it is, and we have to act in conformity with it, experience having taught us the disastrous consequences which must result from erroneous ideas regarding it. Is it safe to imagine that we can evolve moral law to suit our desires, and control it better than we can the law of gravitation? Or does not the teaching of history rather show that to be prosperous a nation must by intelligence and experience learn what are the moral laws ordained by God, and act in accordance with them or suffer disaster as certainly as disregard of the law of gravitation entails disaster?

The statement that 'a man's mode of action should be determined by his own character' may delight all jail birds as being their justification; but it is not by such characters

that civilised government can be maintained on any part of the face of the earth.

6th July, 1909.

In my letter of 17th April I postponed allusion to an important scientific question which suggested itself in connection with Professor Threlfall's lecture.

I had intended to record some of his arguments for the purpose of basing on them a suggestion to the effect that the force of gravitation by which the constituent parts of each chemical atom are held together limits the size of each atom, because beyond a certain size, differing in each chemical element, the pressure of gravitation becomes too great for the substance to bear without crushing itself with the increasing stress resulting from its own growth. That seems to be a natural consequence of the arguments alluded to in my letter of 26th April. The thread of my intended argument has, however, faded away together with memory of those given in Professor Threlfall's address on which it was based. I therefore abandon that route, and do so without much regret, because I hope before long to reach the question from another direction.

12th July, 1909.

I do not wish to expand the comments given in my letter of 30th June in support of the idea that moral law, like that of gravitation, exists quite independently of man's existence; so that they would be what they are even if human beings did not exist, and our part is merely by experience and intelligence to learn what they are and conform ourselves to them or suffer disastrous consequences. There are, however, some points in the Cambridge publication which may incidentally be looked at from that point of view.

Dr. Jane Ellen Harrison, in her contribution to the Cambridge Essays, dismisses the Mosaic account of the creation of the world so completely as to declare that the very word 'Creator' has nowadays almost passed into the regions of mythology, and that we now have instead of it *L'Évolution Créatrice*. That same voice from Newnham College tells us that we should look on religion 'not as a *réalité faite* but as a process.' Is Newnham on the right track? And, even if so, is she not trying to lead us too fast?

If we turn to Professor Haeckel's essay, also in the Cambridge publication, we find he tells us that the united laws of the constancy of matter and the conservation of energy are necessary for the prevalence of the principle of evolution throughout the universe. That is precisely the view taken by Herbert Spencer, and I will not say that Professor Haeckel is not entitled by any of Darwin's writings to claim him as belonging to that school. But what does that school, for which Haeckel now claims Darwin, mean? It means that the material universe, being eternal, wants no 'creator'; it wants no God; it is self-sufficient; and, according to that school, it does not recognise any law outside itself as having brought it into existence.

It is, however, a relief to see that Cambridge is not altogether enveloped in the darkness of that philosophy. The wider view of evolution which regards matter itself as evolved from a pre-existent immaterial force, is not only more or less explicitly adopted by some of the essayists, but Darwin is *claimed by them* as belonging to that school which brings in an antecedent cause by which the material universe has been created, and is, therefore, neither its own creator nor its own God.

The fact is that Darwin belonged neither to that school of evolutionists nor to that of Herbert Spencer. He never had any conception of the vast importance of the question

at issue between them. Professor Schwalbe, of Strasburg, in his contribution to the Cambridge essays, records Darwin's written statement in 1878 to the effect that 'he never troubled himself about such insoluble questions as the eternity of matter'; and that is just what I understood to be his attitude when I gave my public lectures at Willis's Rooms in the year 1877.

It has in fact always appeared to me that Huxley's attitude towards Darwin was an admirable example of what might have been Darwin's attitude towards St. Hilaire and Lamarck if he had fully appreciated their ideas of evolution when he published 'The Origin of Species.' The manner in which some of the Cambridge Essays attribute to him a more extended view of evolution than held by Lamarck, St. Hilaire, or Robert Chambers (instead of merely a modified explanation of its action) is quite at variance with the evidence which has impressed itself on my mind.

At the time I gave the lectures in 1877, above referred to, I was told that Charles Darwin declared himself unable to understand Herbert Spencer's ideas of evolution; and that is thoroughly confirmed by his own words written the following year, as quoted by Professor Schwalbe.

Neither Spencer nor Darwin recognised the vast importance of the difference between the idea of the eternity and that of the evanescence of matter. Their schools never opened that book, and those who are now attributing to Charles Darwin scientific views in advance of those which he really held are doing him injustice from a higher than a merely scientific point of view.

The philosophy which the Cambridge Essays show may, with the reverse of offence to its leading spirits, be termed the godless school, is to-day wielding widespread influence in the social and in the political, as well as in the

scientific world, notwithstanding that it has always been instinctively opposed by many earnest and learned men. The broader view of nature which has made itself heard in some of the Cambridge Essays now reinforces those instinctive feelings by showing the godless philosophy of Herbert Spencer's school to be really narrow-minded and unscientific.

17th July, 1909.

The tides formed a prominent subject in Part III. of the series of letters I am addressing to the Royal Institution, and the general tenor of Sir George Darwin's contribution to the Cambridge Essays makes this an opportune moment for some comments on a closely allied subject.

In the work on Oceanic Circulation which I published in the year 1866, I showed that the ocean is in fact divided into eight principal districts and two subordinate antarctic districts, and explained that the current which surrounds each principal district tends to describe an elliptical course, having its focus in the eastern part of the district on the surface of the ocean, while the elliptical course in the understrata has its focus in the western part of the district; and that the elliptical orbits so described result from the combined action of two forces, one tending to cause a horizontal and the other a vertical circulation. The diagrams I then published, showing those districts in plan, in section, and in cross section, give the main features of oceanic circulation; and the forty-three years which have since elapsed have brought such confirmation from all parts of the ocean as to make the existence of the circulation they show no longer a question for reasonable dispute. I then suggested that the action by which the planetary orbits are made elliptical must be analogous to that by which elliptical orbits are created in the ocean.

There was never any misunderstanding as to the current creating action of vis inertiæ on which those oceanic districts are based being at variance with Newton's definition of that force and the so-called First Law of Motion based on it; and it is now of importance to notice that by applying to the planetary orbits the arguments based on the course of the ocean currents, not only is an explanation given for those orbits being elliptical instead of circular, but also the argument requires for its own confirmation an inclination of the planes of the orbits in relation to the plane of the sun's equator, thus explaining that phenomenon as well as the ellipticity, whereas Newton's 'Principia' gives no indication of those planes being in any manner dependent on that of the sun's rotation.

The discarding of Newton's First Law of Motion in oceanic phenomena naturally precludes its use in astronomy, and the ellipticity of the planetary orbits therefore called for a cross action of gravitation in the heavens. As regards that cross action, in the years 1870 to 1873 I showed one of the forces to be a transverse action of the sun's gravitation which endeavours to revolve the planets in circles in the plane of the sun's equator, and the other force, acting across the sun's force, is dealt with in a Paper entitled 'The Scrapping of the First Law of Motion,' which I presented to the Institution of Civil Engineers on the 9th January, 1907, and also in some previous writings.

To make the position of the subject clear it is well to note that I had in the year 1854, or shortly before that, declared that so-called law not to be true in Natural Philosophy, but it was not until some years afterwards that I got the idea that its validity might be brought to issue in the question of oceanic circulation; so that in the first instance I took up the latter subject, not for its own sake, but for the purpose of testing the so-called law. The result of the oceanic

explorations conducted during the past forty years has been to show the actual movements of the ocean water to be at variance with that so-called law, and that refutation invalidates it in astronomical as well as in terrestrial phenomena.

My present return to the subject is called for by Sir George Darwin's contribution to the Cambridge Essays, in which, among other points of special interest, he tells us that if we 'imagine a satellite and its planet, and consider each of them to be of indefinitely small size, in fact particles —then the satellite revolves round its planet in an ellipse.' Sir George Darwin treats the elliptical orbit as the normal form, and this is arrived at by treating the motion when once started as tending to maintain itself for ever.

That does not give any explanation of the actual deviation of the planes of the planetary orbits from that of the sun's equator; whereas the admission of the transverse action of the sun's gravitation gives a continuously acting motive force along the planetary orbits, and a similar transverse action exerted by a distant star, holding a position in the stellar system similar to that held by the sun in the solar system, explains the inclination of the planetary orbits to the plane of the sun's equator.

21st July, 1909.

Perdita seems to be a long way from having finished the story she has to tell.

It is well known in the engineering profession, and, for aught I know to the contrary, among biologists also, that the best possible shape for a horse of fourteen hands cannot be the best for one of fifteen hands if they differ in no respect but size, because the weight to be carried depends on the cube of dimensions and the strength of limb on the square. Therefore any mere increase of size makes an

animal too heavy for its limbs if previously perfect. For that same reason it has long appeared to me that the fossil remains of animals vastly exceeding in size any now living may be regarded as practical evidence to the effect that the earth in former times rotated on its axis faster than it does at present. That faster rotation of the earth would have enabled animals which under present conditions would be unable to support the burden of their own weight to move about the equatorial regions of the earth with ease and comfort. Huge animals dominant in those regions under the faster rotation might have ceased to be able to defend themselves against the invasion of others from higher latitudes after the arrival of Perdita had slowed down the earth's rotation to its present period.

As regards the idea of the moon acting as a drag on the earth's rotation, the position to-day is that Newton's ' Principia ' does not admit any such dragging action because it is incompatible with his first law of motion; but it is nevertheless admitted by some leading astronomers who avow allegiance to that so-called law. My contention is that Newton's law of gravitation necessitates the existence of that dragging action, and also that it just as certainly excludes the so-called first law of motion from the realms of Natural Philosophy. From my point of view, therefore, the arrival of Perdita to become a companion to the earth reasonably explains the fact of the antediluvian monsters having ceased to live.

22nd July, 1909.

The geological evidence to the effect that in prehistoric times there must have been stronger tidal action on the earth's surface than at present seems at first sight to form an important argument against the story so far told by Perdita, as in fact the moon creates about two-thirds of the

present tidal action ; and it might therefore at first sight be supposed that before her arrival the tides would have been three times *less* than they now are. Such conclusion would, however, be erroneous.

If we suppose, for the sake of illustration, that the retardation dealt with in my letter of the 21st instant reduced it to one-half of its previous velocity, then the sun's tidal action, increasing as the square of the velocity, would be four times greater than at present, so that the regular solar tide would have been one-third greater than the high spring tides produced by the combined action of the sun and moon now are.

Added to this they would when breaking against coast lines exert a force increased as the square of the velocity, or four times greater, making them as 16 to 3, or more than five times greater than the present force of tidal action.

I give the above merely to show that in order to account for higher tides in prehistoric times than we now have, it is not necessary to suppose that the moon must then have been nearer.

23rd July, 1909.

The fixing of the date of Perdita's arrival forms an interesting problem from many points of view. Besides its connection with some epoch-making event in the history of the solar system, which gives it astronomical importance, it seems impossible that it could really have occurred without leaving indelible traces in our geological records. Does Perdita's story solve any riddles in those records ?

Mr. Walter Bradford Woodgate in his work ' A Modern Layman's Faith,' published in the year 1893, makes an elaborate analysis of the records of the Deluge given in the Biblical narration, in the Chaldean Genesis, in the record given by Berosus, and in the Book of Jasher ; and he

interprets the evidence as being to the effect that before the Deluge the earth's axis was upright, or nearly so, in relation to the ecliptic, as that of Jupiter now is; and he points out that, under those conditions, our present change of seasons from summer to winter could not exist, and the first Monsoon in the history of the world must have occurred with the Deluge.

He suggests that the clear and mistless sky to admit the full rays of the sun had not been beheld on earth until after the Deluge; and that the alternations of season giving seed time and harvest then first became established as a terrestrial phenomenon. The rainbow described after the Flood he considers to have been a new phenomenon to the inhabitants of the world, a clear view of the sun having been previously prevented by the existence of a constant mass of vapour forming the water of the firmament above the earth.

On the supposition that the wrecking of Perdita and the change of the inclination of the earth's axis were both consequences of the same perturbation of the solar system, and subject to the tidal action suggested in my letter of 22nd instant, Mr. Woodgate's interpretation of the Mosaic record fairly accords with the idea of our moon being the planet Perdita; and his argument as to an abrupt cessation of the higher tides in the Quaternary period of geology would make that the date of her arrival in our terrestrial regions. The establishment of that date is certainly an interesting question, and Mr. Woodgate's arguments give, at least, a plausible basis for its discussion. The Biblical argument combined with that tentative geological evidence, shows mankind to have lived on earth at the same time as the huge animals now extinct whose bones remain to astonish all visitors to our Natural History Museums. Mankind, however, then probably knew as little of the equatorial regions and its huge-sized inhabitants as we knew of the western hemisphere before the voyage of Columbus.

30th July, 1909.

Mr. Woodgate's argument as to an abrupt cessation of higher tides in the Quaternary period of geology (alluded to in my letter of the 23rd instant) is to the effect that ancient tides seem to have had force enough to swill out the flats now known as water-meadows far above the present limit of tidal influence in the Thames Valley; that the gravel beds in those flats seem of themselves clear evidence of tidal action, as the gravel testifies to tidal ebb and flow, and not to fresh water flow in one direction (because by the latter action the gravel would be sorted, whereas it is in fact in each bed practically homogeneous); that similar water-meadows in the Severn Valley and also in France, in the valley of the Somme, show the same tidal action high above the range of the present tides; and that the cessation of tidal action in those meadows was not a matter of graduation, but was a somewhat abrupt transition from Tertiary great tides to those of modern dimensions.

In connection with Mr. Woodgate's clearly defined arguments from which I have extracted the foregoing, it is of interest to turn to some views expressed by Sir Roderick Murchison, which I quote in his own words. In his renowned work 'Siluria' that gallant and brilliant Highland soldier says: 'I never examined any extensive area without recognising evidence of fracture, displacement and occasionally inversion of the strata, which no amount of gradual, continuous action could possibly explain. . . . How account for the clean sweep of denudation which has been made of wide tracks? . . . How account for the tons of fractured bones of hippopotami, huddled together in crevices and caverns of the rocks of Sicily, except by some such violent action? . . . What more natural method of explaining those facts than by referring them to the translation by vast bodies of water suddenly put into action by those upheavals of parts of the earth of which we have clear evidence? . . .

I only beg to reassert the belief I have long entertained from evidences before me that those convulsions were infinitely more powerful than any one of which history records an example.'

Sir Roderick published the above in the year 1867, in repetition of previous publications, and, in a work which I published eighteen years afterwards, I called attention to some old historical records which Murchison overlooked—as did also his great geological opponents Lyell and Charles Darwin. In my work, 'The Ocean,' I made the following quotations from the Book of Genesis and the Book of Enoch: 'In those days Noah saw that the earth became inclined.' 'The earth labours and is violently shaken.' 'The foundations of the great deep were broken up.' 'And the foundations of the earth became equalised, while other depths were opened into which the water began to descend, until the dry ground appeared.'

The rough work told of in those old historical records is quite incompatible with the smooth geology of Lyell, Playfair, and Hutton; while, at the same time, it harmonises with, and justifies, Sir Roderick Murchison's interpretation of our geological records. The effect of the slowing down of the earth's rotation as the moon arrived would be a tendency of the equatorial regions to sink, and of the polar regions to rise. And also, whatever the inclination of the earth's axis to the plane of the sun's equator may previously have been, that inclination would have to be readjusted to the changed velocity of rotation. If the axial rotation ceased altogether then the axis, being the shortest diameter, would point to the sun.

Thus at the moon's approach the inclining of the earth's axis recorded by Noah would be a necessary consequence of the retarding of the earth's rotation caused by the moon; and the 'violent shaking' would naturally result from the

effort of the earth's crust to adjust itself to the slower rotation.

Sir Roderick readily admits that gradual uprisings and sinkings of lands have occurred in parts of the earth during the historic period ; but, at the same time, maintains that formerly acts of much greater violence in the dismemberment and denudation of the strata took place than in any changes during our era. I venture to suggest that Noah's story of the Deluge (*pace* Cambridge) is well within the historical period, and that the Uniformitarians are fighting a losing cause in their attempts to show that those historical records are not confirmed by our geological records.

Perdita disturbed the normal course of terrestrial history ; and gives Sir Roderick Murchison a complete victory in his hard-fought battle against his able and learned opponents, Sir Charles Lyell and Charles Darwin.

4th August, 1909.

In his ' System of Chronology,' published in the year 1784 in Edinburgh in rivalry of the renowned French work ' L'Art de Vérifier les Dates,' Dr. James Playfair, a contemporary of the great geologist John Playfair, tells us, as the first statement in his ' Series of Remarkable Events ' :

' 4008 B.C.—The world was created in autumn.'

It is remarkable that a hundred years after Sir Isaac Newton had elaborately pointed out that the authority on which that statement is based is not always reliable, so eminent and learned a scholar as Dr. Playfair should have based the above and other historical statements upon it without any collateral evidence. And that was done while in the same city of Edinburgh John Playfair was expounding the geological evidences which were then gradually revealing the true grandeur of the earth's history.

It was only about fifty years after Galileo had been persecuted on the supposition of his astronomical views being at variance with Biblical writings, that Sir Isaac Newton formulated his arguments which cut at the root of the ideas held by the persecuting authorities ; and those arguments are tersely and vigorously given in so uncompromising a manner that it seems as if Newton were determined that the prestige of his renown should be unmistakably available against recurrence of persecution on any similar basis. The more enlightened view which now prevails regarding those old writings does not diminish their importance ; but, on the contrary, in face of the marvellous revelations of geology and astronomy their value is enhanced.

A cause for the earth's change from an upright to an inclined axis as recorded by Noah has been fairly given by Perdita's story.

In a work which I published in the year 1877 (' The Winds and their Story of the World ') I suggested that the internal heat of the earth is due to the stress and strain of its axial rotation ; and if that is so, then a reduction of the rate of rotation at the time of the Deluge to half its previous velocity would tend to cause a reduction of internal heat to one-fourth of the former temperature ; and that accords with the fact of the Carboniferous periods of geology having occurred *before* the Deluge ; because the greater warmth would have encouraged a luxuriant growth of vegetation on other conditions being favourable. On the other hand, if, for those same reasons, we infer that the Glacial periods should have occurred *after* the Deluge, we shall be forced to the conclusion that mankind existed on the earth in a civilised state long before the Glacial periods, and that the Deluge occurred, not in the early Quaternary, but in the later Tertiary period, as geology seems to indicate a gradual reduction of the earth's temperature at that time. It does

not appear to me that Mr. Woodgate insists on the great tides of the Thames' Valley having been continued *after* the commencement of the Quaternary period; and the arrival of Perdita may perhaps give the best line of demarcation between Tertiary and Post-Tertiary times in geology. I have not so far met with anything that is absolutely at variance with the story she tells.

13th August, 1909.

As in my letter of 4th instant Perdita's story became connected with chronology, it reminds me that in the first year of this century—that is to say, in the year 1900—I offered to the Royal Institution a Paper showing that those who suppose the year 1901 to be the first year of the twentieth century of the Christian Era are mistaken. In afterwards publishing the Paper I spoilt it as a connected essay by amplifications which, however, made it a more important publication than at first intended; and it seems not out of place now to recapitulate the corrections it gives on several points which have been accepted as facts by leading authorities in chronology.

The most useful of modern works on chronology—that by John J. Bond entitled 'Rules for Verifying Dates,' gives three different calendars in juxtaposition to serve as a reference for collating dates recorded by the respective systems.

As regards those three calendars, I have shown (1) that the one supposed to have been framed in Greece by Meton in the year 432 B.C. is in fact a calendar which must have been framed by some of the early Christians with the year of the Crucifixion as its epoch; (2) that the one which Bond records as having been used in old Saxon Charters after 463 A.D. is that which belongs to what was formerly known as the True Era of Christ in contradistinction to that introduced by Dionysius and termed the Vulgar Era; and

(3) that the one said to have been specially framed by Dionysius for the Christian Era in the year 532 A.D., and of which the Golden Numbers are now in use in our Prayer-book, is in fact Meton's original calendar; our Era having been made to commence with Meton's Golden Number I. most nearly approximating to the date of the birth of Christ.

It is clear that the calendar framed for the True Era of Christ was used by the early Church in England, and that of the Crucifixion perhaps by some of the Eastern Churches; but if in Rome a calendar specially framed for the Era of Diocletian was in use before the Dionysian reform, that calendar was then rejected; merely perhaps because it was offensive to all Christian Churches.

The calendar of the Era of Christ as it appears in Bond's table was merely Meton's with the Golden Numbers transposed so as to make Number I. commence with the birth of Christ. Dionysius put back the Golden Numbers to their original positions and readopted Meton's calendar as originally framed on the 16th July, 437 B.C.; and he made the Christian Era begin with the first Golden Number I. which occurred after the birth of Christ. The idea that Dionysius made the era commence three years after the true date 'by mistake' cannot, I think, any longer be entertained by anyone who studies the subject. The result, in combination with the Gregorian Reformation, is that by adding 753 to our A.D. years we have our Era identified with that of Rome as reformed by Cæsar; and also our cycle of Golden Numbers carried back 123 Metonic Cycles from the new moon of 26th July, 1900, fixes the epoch of Meton's calendar at the new moon of 16th July, 437 B.C.—not 432 B.C. as generally supposed; so that our Era is in fact so arranged as to allow our calendar to run in simple continuity through Grecian history.

The subject has been intricate, but I do not think the foregoing conclusions likely to become disestablished. At any rate, the argument of my published work has thrown a light into the subject by which entanglements of the Dark Ages may be unravelled even if I am not correct on all points.

14th August, 1909.

I have before me several of Queen Victoria's bronze pennies: one issued from the Mint in 1863, one in 1867, and another in 1870. Also silver shillings issued in 1868, 1872, and 1873. And a gold sovereign issued in 1871.

All these coins when they were issued represented Double Standard currency—that is to say, their value fluctuated with the mean increase of abundance or scarcity of gold and silver, not with that of either one of the two metals.

In the year 1874 they ceased to be Double Standard coins, and ever since that they have been Gold Standard coins.

In the Preface to the Third Edition of my book, 'The Standard of Value,' published in 1883, I alluded to that change as tending to make the rich more rich and the poor more poor. Over and over again I have in the meantime seen arguments or statements in various publications evidently originated by my above statement and supposed to be its refutation, but in fact showing a complete misunderstanding of the position of the question. The theory of that question is sufficiently dealt with in my above-mentioned work, and I will have nothing more to do with it; but I now place before you the following statement which I extract from a book published this year by Mr. Andrew Carnegie, entitled 'Problems of To-day.' Mr. Carnegie has, as we all well know, been one of the largest

employers of labour on either side of the Atlantic. He says : ' The extremes in the distribution of wealth have never been so great as they are to-day.' That opinion of Mr. Carnegie's is well supported in the London Press.

Perhaps you will say that the present position has arisen from other causes than that which I supposed to be tending to produce it. I will not re-argue ; but here is another point. In my Paper read before the English Literary Society of Buenos Ayres on the 28th July, 1880 (29 years ago), I pointed out that ' an enhanced value of gold which would be equivalent to an addition of more than a thousand millions sterling to the debts of England and France would add less than one hundred millions to the debt of Germany.'

Will you, Sir William Crookes, or will any man with any pretension to acquaintance with the subject, by any mental effort persuade yourselves that the relatively greater handicapping of England and France in the meantime has not assisted in creating a relatively greater increase of wealth and prosperity in Germany ?

In the same Paper just quoted I argued that ' we may perhaps safely consider 20*d.* as a practical limit to the fall in silver which the attempt to establish gold monometalism might cause.' And then follows the now important point : ' It must, however, be borne in mind that, though the relapse of some nations to inconvertible paper might partially check the immediate rise of the value of gold, their attempts to return to specie payments would tend to cause a slower rise ; so that in theory there is no limit to the rise in the value of gold which a demand for metallic circulation might cause, and we have therefore before us the abstract possibility of gold being trebled in value.'

We are now in the second phase of the position above described. When I published that diagnosis the price of

silver was about 51*d.* per ounce, soon afterwards 52*d.* or 53*d.*, and on 26th November, 1902 (twenty-two years afterwards) it was quoted 21$\frac{11}{16}$*d.*—not so very far from the suggested possible fall to 20*d.* In recent years the position has been prevented from becoming insupportable by unexpectedly large discoveries of gold ; but the full benefit which might have accrued from the new supply is thwarted by our action in forcing a Gold standard on India, which that country does not want. We are destroying their Silver standard to their detriment and forcing gold on them to the detriment of our own industrial classes. The arbitrary manipulation of the gold standard in India is open to dangerous abuses, and is at variance with the only natural principle, under which the London and the Paris Mints ought in 1873 to have combined to maintain the price of 60*d.* They can, whenever they choose to do so, restore that price of 60*d.* and thus redeem the financial honour of Europe, even if the United States refused to join in the combination. The industrial and agricultural classes of the world are entitled to have silver included in the standard of value, and must have it to avoid disaster all round. It was a mistake on the part of London in 1870 and of Paris in 1873 to allow control of the metallic currency to pass from them through, in each case, suppressing instead of acting on a law then available ; but by joint action that control can be recovered, though, instead of the simple and easy matter it then was, it has become the biggest business the financial world has ever had to face. The industrial world is working uphill with the summit out of sight, and it is an artificial hill which, as pointed out in the above quotation, may become very steep. Man must work to live ; but common sense calls for the removal of artificial obstructions which prevent a due result of wealth and comfort from accruing in response to our efforts.

16th August, 1909.

I supplement my letter of 13th instant for the purpose of pointing out that where I say that the calendar which Bond gives as having been specially framed by Dionysius for the Christian Era in the year 532 A.D., and of which the Golden Numbers are now in use in our Prayer-book, is in fact Meton's original calendar, I should have added that is so only as regards those Golden Numbers.

That emendation allows me to add two more points to those recorded in my letter of 13th August. (1) The new moons which Bond gives as having been adjusted by Dionysius to our Golden Numbers are *not* the real moons belonging to those Numbers either at the commencement of our Era or at the time of Dionysius; but they *are the real moons* which belonged to the Numbers during the reign of Diocletian. (2) Gibbon records the election of Diocletian as having occurred on the 17th September, 284 A.D. which is incompatible with the calendar of Dionysius if we suppose Diocletian to have made the calendar of his Era commence with Golden Number I. Gibbon, however, says the dates of that time are 'very doubtful chronology'; and it therefore is worth while to notice that if we suppose the epoch of Diocletian's Era to have been in the year 285 A.D. (instead of 284), then it becomes mere matter of fact that the calendar as given by Bond is one which is correctly adjusted to the Era of Diocletian. And, therefore, that calendar as given by Bond seems to me to make it most probable that 285 A.D. is the true date of Diocletian's epoch.

It is easy to be misled in dealing with these calendars, but when carefully traced they are strong evidence as regards disputed dates. Bond's courage in dealing with them seems to me to be worthy of the highest praise, and constituted a first step towards getting them out of the confusion in which they have been involved.

20th August, 1909.

In my letter of 14th instant I gave an extract from Mr. Andrew Carnegie's work ' Problems of To-day ' to show that the course of events has justified the general tenor of the views expressed by advocates of the Double Standard as an urgent practical question thirty years ago, and long before that as an important and interesting academic question. The main argument of my work on the standard of value shows how my letters of 13th and 16th instant, unravelling confusions connected with history in the Dark Ages, naturally lead to a consideration of the currency question dealt with in my letter of 14th instant, because I have there contended that a change in the value of Roman coins was a principal cause of the disorganisation of government which led to the Dark Ages.

Attention has recently been freely directed to the fact that in the falling days of Rome the extremes of wealth and poverty were very great, and perhaps Mr. Carnegie did not intend his statement to cover time so far as to the state of affairs in the Roman Empire immediately before the Dark Ages. Gibbon records that there was then in Rome, ' increase of taxes ' and ' relaxation of discipline,' and that ' the agriculture of the Roman provinces was insensibly ruined ' ; but he has not, I think, recognised the state of affairs as being a natural result of the disorganisation of the Roman coinage. McCulloch, however, tells us that an artificial enhancement of the value of the gold coinage was effected in the worst days of the Roman Empire ; and he further says that ' equal injustice is always done to the poorest and not least numerous class of society by increasing the value of money as is done to the wealthier class by its depression.' And David Hume says : ' A nation whose money decreases is actually at that time weaker and more miserable than another which possesses

no more money, but is on the increasing hand : the poverty and beggary and sloth which must ensue are easily foreseen.'

I am not going to repeat arguments already published, but venture to suggest that Lord Goschen's change of opinion regarding the Irish Land Act of 1881, as recorded in my Chapter on that subject, makes that short Chapter a very practical object-lesson as regards the difficulties which agricultural and industrial pursuits of all descriptions now have before them. It is clear that neither he nor any of those responsible for the Act properly understood the problem they had to deal with in connection with the land question. It must, however, be noticed that it is easy to say, as Lord Goschen (then Mr. G. J. Goschen) did six years afterwards, that the Act was 'a gigantic error,' but the fact is that *something* had to be done : that was peremptory : it was clear that the position would otherwise have become chaotic ; and it seems to me that the only course worthy of the country then was, as it now is, the restoration of silver to its former position in the standard of value.

The civilised world has all along been entitled to look to London for a lead, though in fact a few years ago a strong party in the United States endeavoured to induce their country to act in our default. Our position is still such as to enable us to act more effectively than any other nation could do alone, and the most elementary principles of justice as well as expediency call on us to do so. Money-lenders may profit for a time by a rising value of the coinage, but its adverse effect on agricultural and other industrial pursuits must in the long run bring new taxation on accumulated wealth, not only in some form of Poor Rates, but also to make up the deficit caused by those thrown on the Rates ceasing to be producers of wealth and contributors to the public revenue. Any long-continued rising value of the currency must be disastrous to rich and poor alike.

27th August, 1909.

The idea that each material atom is a miniature Solar System and that corpuscles emitted by radio-active substances were normally moving in circles inside the atom with the same velocity as that with which they escape from it, has not been absolutely rejected by Sir Joseph Thomson in his Winnipeg Address, but it may almost be said that it was merely glanced at askance and played so subordinate a part that I venture to think we may safely consider that those wondrous little systems have virtually been abandoned by their most renowned supporter and that the scientific world will before long cease to hear much more about them.

The idea of those infinitely small ' solar systems ' had taken such hold of the public fancy that Sir Joseph Thomson's frankness and courage in abandoning the popular little things seems worthy of the highest praise and tends to give confidence in his sincerity as he continues unflinchingly to champion ' the ether.'

Supported by all the prestige of the Presidency of the British Association, he tells us that : ' The ether is not a fantastic creation of the speculative philosopher ; it is as essential to us as the air we breathe . . . space must contain something which, like ordinary matter, can store up energy which can carry at an enormous pace the energy associated with light and heat, and which can in addition exert the enormous stresses necessary to keep the earth circling round the sun and the moon round the earth.' That summary of the marvels which ' the ether ' has to perform is followed by the avowal that ' the study of this all-pervading substance is perhaps the most fascinating and important duty of the physicist.'

As regards the ' enormous stresses ' which keep the planets revolving round the sun it must not be forgotten that Newton demonstrated the enormous stress of the reciprocal

action of gravitation between the earth and the sun, and forty years ago his law of gravitation was shown to supply also the revolving force which carries the earth onwards in its orbit, so that the introduction of any stresses for that purpose beyond those given by gravitation is like adding a fifth wheel to a coach ; and as regards heat and light I have long ago shown that they do not call for any connecting link between the earth and the sun but the clearly recognised and acknowledged action of gravitation. The so-called ether pressure in space is in fact nothing but the opposite aspect of astral gravitation which pervades the universe. 'The ether' is not wanted for any of the purposes specified at Winnipeg.

And now I address through you, Sir William Crookes, not only to the Royal Institution but to the scientific world, an earnest protest against Sir Joseph Thomson's statement that: 'Before the discovery of radium it was supposed that the supplies of heat furnished by chemical changes going on in the earth were quite insignificant, and that there was nothing to replace the heat which flows from the hot interior of the earth to the outer crust.' The fact is that condition was supposed to exist only by those who knew no better, but thirty or forty years before the discovery of radium I had pointed out that the stress and strain caused by the earth's axial rotation as that motion is resisted by the dragging action of astral gravitation must create heat in the interior of the earth, and that the creation of that heat must be as constant as the velocity of rotation on which it is dependent. The creation of heat in the interior of the earth is just as constant as its flow to the outer crust.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

To Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers

21st August, 1909.

Dear Sir,—I have to-day received the new volume of our Proceedings, and find it expedient to submit some comments connected with the contribution to the discussion of the Papers on Railway Curves which I addressed to you on 27th January and have since published in Part. III. of 'The Sling.'

1. Mr. Shortt (p. 139 of the Proceedings) alludes to Mr. Spiller's paper as having explained that the motion of a car going round a curve 'might be considered as consisting of a straight roll forwards together with a horizontal rotation.'

When writing that Mr. Shortt has mixed Mr. Spiller's views with my contributions; for, in fact, Mr. Spiller's Paper dealt only with the so-called horizontal rotation and I call attention in my letter of 27th January to the combination of the two motions specified by Mr. Shortt simply because the 'straight roll forwards' was ignored not only in Mr. Spiller's Paper but also by all (including Mr. Shortt) who spoke in the discussion. The arguments in the discussion, as well as in the Paper, were incompatible with the idea of the existence of a vertical rotation being just as true, or just as false, as that of the horizontal rotation dealt with by Mr. Spiller and by Mr. Shortt. The existence of the vertical 'rotation' destroys the basis of the arguments given by them at the meeting regarding the

'horizontal rotation.' Mr. Shortt attributes to Mr. Spiller what was in fact expressed by me in exposure of Mr. Spiller's mistake; and he treats it seriously, whereas I gave it *in reductio ad absurdum*.

2. I notice that the attribution of the so-called horizontal rotation to Sir Isaac Newton has been wisely suppressed in your report of the discussion.

3. In my letter of 27th January I gave an explanation of the derailment of a Railway Engine, regarding which I understood Mr. Jacomb-Hood to appeal to his hearers for help to clear a mystery; but instead of that appeal there now appears (on page 122 of your report) his statement that the phenomenon was 'owing of course to unequal loading of wheels due to super-elevation of the outer rail.' It seems strange that I should have mistaken that statement for an appeal for an explanation; but there is, to say the least, no harm done by my mistake, as I feel confident that the explanation I have given will in due time be accepted by all engineers; that given by Mr. Jacomb-Hood in your report being merely a small step towards it.

Scriptae manent.—'The Sling,' Part III., published two months ago, and your report just issued. On both the above points at issue I am quite satisfied to wait for a verdict. They might perhaps be supposed by some readers to be trivial questions, though they really are not so, but have issues of far-reaching importance dependent on them.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

To His Majesty's Most Honourable Lords Commissioners, The Admiralty

4th September, 1909.

My Lords,—You have not responded to my recent appeal as regards the tide waves of the Great Southern Ocean.

In a work which I published in the year 1868 I pointed out that the main tide of that ocean flows *eastwards*, not westwards, though all scientific authorities supposed the westward motion to be not only required by all accepted theory but to be also a well-ascertained matter of fact.

The eastward motion is not only a correction as regards an important matter of fact, but it also involves the rejection of what has long been accepted in science as the First Law of Motion.

That position of the question was made clear not only in the work just above mentioned but also in an abstract which I submitted to the Secretaries of the Royal Society that same year.

The following points, in sequel to the above, show the great importance of the question and that it was misjudged by the Royal Society, who rejected my views on the ground of their being at variance with generally accepted theory (as I well knew them to be) and also at variance with well-established facts (which they are not). The supposed direction of the tide waves of the Great Southern Ocean I selected from among those so-called facts as easy to be brought to a clear practical solution. In the instructions

given to Sir George Nares when the 'Challenger' was commissioned for her memorable cruise, the Admiralty suggested 'the phenomena of the tides' as calling for his special attention, and it seems now quite time for an official record of the result of the explorations as regards that point: the most important tidal question then at issue.

That tide is a detail of the circulating action of gravitation which I submitted to the Council of the Royal Geographical Society in the year 1866 as a solution of the main features of oceanic circulation, but I selected it as a practical test because it can easily be brought to a clear issue.

I now pass to another result of the rejection of my Paper by the Royal Society.

Sir George Stokes, in whose hands the decision as regards the Paper was left, not only resolved to adhere to the theoretical law which I questioned, but woke up to the *fact* (for fact it certainly was) that the so-called law had never been given practical application in considering what shape ought to give least resistance to a ship's motion through the water. William Froude and others then, under the guidance of Sir George Stokes' mathematical demonstrations, brought about a revolution in accepted ideas on shipbuilding, and all our battleships were shaped under their new principles until after the launching of the 'King Edward VII.'

In a letter dated 12th January, 1903, addressed to the Institution of Civil Engineers, I formally challenged the principles by which our Naval Constructors were then guided, thinking that the best way to bring the question to your Lordships' attention, and on 3rd May, 1905, I addressed your Lordships direct on the subject. Every one of the battleships which your Lordships have launched since the date of that letter, or that of 12th January, 1903, above mentioned, constitutes a practical admission to the effect that the so-called First Law of Motion by which Stokes and Froude had been guided in previous designs is false: that is

to say, it is not true to nature, and in attempting to conform to the indications of that theoretical law all our pre-'Dreadnought' ships had been given too deep a draught in proportion to their displacements.

The theory in question is dealt with in my letters to the Institution of Civil Engineers published in my book 'The Sling,' Part II., and perhaps more concisely in the letters I addressed to your Lordships during the building of the 'Dreadnought.' In publishing Part III. of 'The Sling' I refrained from mentioning the subject of shipbuilding, merely because so serious an international position had been created as to make the exposition of mistakes disheartening, and apparently unnecessary in the belief that your Lordships already clearly understood them; but the latter point now seems doubtful, and there does not now appear to be such call for reticence.

I have the honour to be,
My Lords,
Your most obedient, humble Servant,
WM. LEIGHTON JORDAN.

The letters to the Admiralty above alluded to are given in the following pages, 239 to 279.

Older Letters addressed to His Majesty's Most Honourable Lords Commissioners of the Admiralty and referred to in the foregoing Letter of 4th September, 1909

3rd May, 1905.

My Lords,—You have just commenced to build what will be the largest battleship we have ever launched.

A lecture delivered in this Institution led me, on the 12th January, 1903, to address a letter to the Council of

the Institution of Civil Engineers for the purpose of endeavouring to lead them to understand, and put a stop to the use of an error in the principles of shipbuilding which was adopted by our Naval Constructors rather more than twenty-five years ago under the guidance of Mr. William Froude, supported by Sir George Gabriel Stokes and others.

I have no means of knowing whether the mistake made in the construction of all the battleships and cruisers we have launched during the past twenty-five years is or is not being persisted in by our present Naval Constructors, but, as courtesy would, I think, lead them to make some kind acknowledgment to me if they had found my criticism correct and were altering their principles in accordance with it, I presume that the mistake is still being persisted in.

The quickest way in which I can make the point at issue quite clear is to take as an illustration the two yachts 'Reliance' and 'Shamrock III.' which competed in the last Cup Race run off New York Harbour, as the manner in which 'Reliance' proved herself to be the better boat in a rough sea, as well as the faster with either light or strong winds, appeared to be a practical confirmation of the arguments I had previously submitted to the Council of the Institution of Civil Engineers.

I do not know the exact differences between the shapes of the two boats; but the descriptions published at the time sufficiently showed what I consider to be the point which gave the 'Reliance' the easier and faster motion, as it is the principle which is at issue, and not the exact manner in which effect was given to it by difference of shape, which I wish to illustrate.

Theoretical considerations on which Mr. Froude's principles were based led him to get as deep a displacement as compatible with stability; his idea being that the *inertia* of the water does not resist the ship's way; and that,

therefore, a fish moving at a steady speed through a frictionless fluid would experience no resistance at all. 'In fact when once put in motion it would go on for ever without stopping.'

In opposition to that view, I have contended that fresh work has incessantly to be done in removing the water to make way for the ship; and the force requisite for that work is as the square of the velocity with which the water has to be moved. If, therefore, one ship has a mean displacement of 10 feet, and another ship 11 feet, then, to move them at the same speed the latter has to lift the water 11 feet in the same time as the former lifts it 10 feet; and the relative forces to be expended for the purpose are, therefore, as 122 : 100—that is to say, for that part of the work to be done in propelling the ship, the one with the deeper displacement requires one-fifth more power than the other.

That appears to me to be the point which gave the 'Reliance' greater speed in all weathers than 'Shamrock III.'

As a rule United States battleships and cruisers draw about 2 feet less than those in our Navy of corresponding displacement, and, no doubt, the mean displacement is less to fully the same extent.

I will not dwell on the manner in which the difference in shape is probably such as to make the ship of the shallower displacement a steadier fighting platform as well as a more roomy and comfortable residence; but respectfully urge upon you that the result of the recent Cup Race is a practical demonstration of my arguments to the effect that our Naval Constructors, and other shipbuilders following their lead, have given our ships an excessively deep displacement, which probably places them at a decided disadvantage in relation to United States ships of corresponding sizes.

25th January, 1906.

I addressed a letter to you on 3rd May of last year for the purpose of endeavouring to show that our Naval-Constructors have for about twenty-five years been guided by false principles which have led them to give our battleships and cruisers an excessively deep displacement which is detrimental to steadiness in heavy weather and to speed in all weathers.

Judging from comments which have from time to time appeared in the daily papers, it seems to me that the explanation I gave for the superiority of the American yacht 'Reliance' over our yacht 'Shamrock III.,' to illustrate the question at issue, is now to some extent recognised. It is, however, out of place to complain that we have to sail an ocean-going boat against a racing machine built for merely near shore sailing; as the behaviour of the two boats in the race made it pretty clear that in a race across the Atlantic 'Reliance' would still more thoroughly beat 'Shamrock III.' And it must also be remembered that the American yacht which carried away the Cup crossed the Atlantic to fetch it.

As the explanation I gave stands not only unrefuted but incidentally confirmed, it seems to me justifiable and expedient to place before your Lordships a statement of the circumstances which led to the introduction of the false principles into our Navy, as that will best enable them to be understood.

In the year 1868 I submitted to the Council of the Royal Society a Paper explaining a theoretical action of forces which tends to carry the main tide waves *eastwards* through the Great Southern Ocean (that is, *against* the course of the sun and moon); and also claiming that records given in the Admiralty Tide Tables confirm that action and show the idea of a great tide wave following the moon round the

Great Southern Ocean, as depicted on Dr. Whewell's chart, to be absolutely at variance with the facts of the case, which have to a great extent been ascertained only since his chart was published.

The Council did not allow that Paper to be officially submitted to the Society because they not only firmly believed that the tides do, in fact, flow more or less as depicted by Dr. Whewell, but also because the denial of the so-called First Law of Motion, which was necessitated by my work, they considered to make my views too wildly erroneous to allow of their being taken into consideration by any serious scientific society.

The result was that, instead of that Law of Motion being discarded as a scientific theory, Mr. William Froude, Sir George Stokes, and others decided to give it a practical application for the guidance of shipbuilders which had never before been attempted, though many scientific men had doubtless been guided by it in attempting to contrive perpetual motion machines on the supposition of that law being true to Nature.

Under the completely new principles which were applied to shipbuilding under the guidance of that false law, our battleships and cruisers have been given such a mischievously deep displacement that I sometime ago suggested to the Council of the Institution of Civil Engineers the expediency of consulting Sir William White (one of their members) as to the cost which would be requisite for re-shaping their bows and sterns in such a manner as to lift the ships higher out of the water.

I urge upon your Lordships that the victory of 'Reliance' over 'Shamrock III.' calls for your most earnest consideration in face of the arguments which I had addressed to the Council of the Institution of Civil Engineers before the race in which those boats competed took place.

30th January, 1906.

On 3rd May of last year and on 25th instant I addressed letters to your Lordships for the purpose of pointing out that, under the guidance of mathematical demonstrations based on a false Law of Motion, erroneous principles of shipbuilding have in recent years been applied by our Naval Constructors.

I do not purpose now to add anything further on that point, but I earnestly claim your Lordships' consideration for a matter incidentally referred to in the latter of my above-mentioned letters.

The complacency with which the reputed leaders of science have, without any attempt at verification of the facts involved in the question at issue, persisted in taking it for granted that Dr. Whewell was right in giving the tide waves on his chart a general westward motion round the world in the Great Southern Ocean, is a consequence of the same bondage to the so-called First Law of Motion under which Sir George Stokes supplied Mr. William Froude with mathematical demonstrations for the guidance of our Naval Constructors.

The idea that the inertia of the water does not resist a ship's way is perhaps the wildest theoretical delusion that ever was seriously entertained in the scientific world; and the idea that the tide waves follow the moon round the Great Southern Ocean as shown on Dr. Whewell's chart is as great a delusion as regards a simple matter of fact.

While, on the one hand, our Naval Constructors have been shaping our ships under the guidance of mathematical principles based on the idea that the inertia of the water does not resist a ship's way, contemptuously disregarding my arguments to the effect that the so-called Law on which their mathematics are based has no practical position in

natural philosophy ; on the other hand, they have considered it beneath the dignity of the Royal Society for a moment to entertain the idea that so startling a fact as a reversal of the supposed direction of the principal tide waves of the world, in open rebellion to that Law, can really be true as a simple matter of fact.

Your Lordships sent out the ' Challenger ' thirty years ago, and the ' Discovery ' more recently, to report on the oceanic phenomena of the far south ; but your experts on both of those ships have been blind to the fact that the main tide waves over which they sailed in those regions are flowing eastwards against the moon. They have considered it beneath their dignity as experts to treat the asserted westward course of those waves as open to discussion. That attitude has been too long persisted in to my disparagement and injury, and urgently calls for your Lordships' serious attention. The fact is that while the tide waves tend to follow the moon round the equatorial regions, and round each polar region in high latitudes, the reaction of the earth's gravitation against the moon's action tends to carry the tide waves eastwards round the temperate zones. The tendency is for the tide wave to be rising *from the west* towards any given meridian in the temperate zones while falling on that meridian and flowing westwards from it on the equator and in high latitudes.

That eastward reaction of the earth's gravitation against the moon's action sweeps the modern fetish which has been given the proud position of First Law of Motion into the limbo of exploded fancies, taking with it the basis of the mathematics by which your Lordships' Naval Constructors have been guided in shaping our battleships and cruisers.

12th February, 1906.

On 3rd May of last year and 25th and 30th January of this year I made bold to address direct to your Lordships letters for the purpose of calling attention to what I had more than two years before endeavoured to show, in letters addressed to the Council of the Institution of Civil Engineers, to be a serious error in the theoretical principles by which our Naval Constructors were then guided.

Yesterday's papers gave accounts of the launching of the 'Dreadnought,' which 'The Observer,' in words all printed in capital letters, calls a 'secret ship.'[1]

There certainly is 'secrecy,' inasmuch as I am prevented from knowing whether the shaping of the immersed part of the ship has or has not been altered from that of our battleships and cruisers now in commission in such a manner as to eliminate the mischievous effects of the error which I have endeavoured to make manifest, but, as representatives of most of the leading nations of the world were present at the

[1] *September*, 1909.—Immediately after the launching of the 'Dreadnought' no serious London paper declared, so far as I know, that she would be able quickly to sink all other battleships, including our own, that might come within her reach, remaining unscathed herself; but pæans in praise of our Admiralty Lords and Naval Constructors went some way in that direction. Fighting admirals were well entitled to plume themselves on her, even if not quite such a terror as the man in the street was led to imagine; and if she really is such an advance from our previous battleships as asserted on behalf of those who took credit to themselves for her production, was it wise on the part of our statesmen to allow our Naval Constructors to parade their skill by obliterating existing navies and making a fresh start for the production of new ones?

All that can be said to the credit of our builders, to the effect of having been so clever as to make older ships obsolete, is to the same extent to the discredit of our statesmen for having allowed such a ship to be built in face of what was then the relative strength of existing navies. The race in 'Dreadnought' building already constitutes a financial disaster throughout the leading civilised nations only because our Naval Constructors were allowed to rush at random into a tempting position without the guiding spirit of a statesman among them. Soon after she was launched I estimated the extra cost involved to this country alone at thirty millions sterling, and a leading naval authority has since publicly estimated it at fifty millions.

launching, our naval competitors are *not* left in the dark, as I am, on that point; and they will have the full benefit of our 'experiment' as soon as the performances of the ship when commissioned are reported. A few months ago I read the report of a speech by a foreign officer which made it clear to me that he had very much the same ideas as I have expressed as regards the mistaken principles of ship-building which have recently been in vogue; and a glance at the ship as she lay on the stocks would suffice to inform him as to whether those principles had or had not been applied in her construction. The difference between his knowing and my knowing is simply that he gives his knowledge to our naval rivals; whereas my knowledge would not have that effect. No national interest can be served by withholding from me, one of the owners of the ship, information which rival navies are given an opportunity for obtaining: a combination of obstinate ignorance with the influential position of Sir George Gabriel Stokes led to the misshaping of the battleships and cruisers we now have in commission; and if our Naval Constructors are endeavouring to strut in borrowed plumes through a 'secret' change of principles, some *gaucherie* may result in national disaster.

20th April, 1906.

My Lord,—The launching of the United States battleship 'New Jersey' leads me to address your Lordship to beg that you will not ignore the letter I addressed to His Majesty's Lords Commissioners on 3rd May, 1905. The dimensions of the 'New Jersey' are published, but mystery enshrouds those of the 'Dreadnought,' so that I do not know whether the principles of construction applied to the battleships we now have in commission have, or have not, been applied to her.

I have pointed out how those principles originated, the point on which they are erroneous, and that the result of the Cup Race between 'Shamrock III.' and 'Reliance' constitutes a confirmation of the views which I had previously expressed in letters addressed to the Institution of Civil Engineers.

You, my Lord, know in what respects (if in anything material) the lines of the 'Dreadnought' have been changed from those previously adopted by our Naval Constructors, and, with all due deference, I submit that, on the one hand, if the old lines have been adhered to a great mistake is being persisted in; and, on the other hand, if my views have been adopted, it is, to say the least, ungenerous on the part of our Naval Constructors to refrain from letting me know that they have come to the conclusion that I have all along been right.

It may fairly be said that until the last thirty years our Naval Constructors were not guided by any abstruse scientific principles, but relied on practical experience. Under the guidance of Sir George Stokes' mathematical demonstrations, based on the so-called First Law of Motion, clearly defined scientific principles were first deliberately made use of, and have greatly influenced the shapes of the battleships and cruisers we now have in commission. Those principles I have shown to be erroneous, and the manner in which our ships make bad weather worse for themselves combines with the victory of 'Reliance' to show that the so-called First Law of Motion is not true to Nature.

I frankly say I am at a loss to imagine any reason for making a secret of the dimensions of the 'Dreadnought,' unless it is that our Naval Constructors have at length in effect adopted the practical requirements of the scientific principle I have for forty years advocated as regards the reaction of astral gravitation against all motion; but as the

advocacy of that principle has handicapped me throughout the best years of my life, it seems incredible that those who have made their positions on the supposition of my views being wildly erroneous can consciously desire to avail of their influential positions to appropriate the credit of my ideas while tacitly allowing it to be supposed that they consider me to be just as much as ever a perverse paradoxer.

If the idea of deep displacement being advantageous has been abandoned in the construction of the 'Dreadnought,' it will before long become ridiculous for anyone to pretend that my crusade against it has not been the cause.

Whatever may be the position as regards the question I have raised in connection with our principles of naval construction, I beg to claim your Lordships' attention to a simple question as to matter of fact in relation to the tides, in which the same principle as that involved in the above question of naval construction is at issue.

In a work published in the year 1868 ('A Treatise on the Action of Vis Inertiæ in the Ocean,' p. 155), I showed that 'Two tidal pivots, one on each side of the equator, move westwards across the ocean, changing their meridian westwards just as the forces which raise the tides change their meridians, and the westward motion in the equatorial regions between those pivots is counterbalanced by the eastward motion in the temperate zones on the other side of the pivots.'

That work requires the tide waves to flow *eastwards* in the Great Southern Ocean in the latitude along which it has been supposed that the tide flows *westwards* all round the world.

I was startled when I found that theory required such *eastward* motion of those tide waves; but, on studying the facts recorded in the Admiralty Tide Tables, I found that they justify the theory and show the main feature of Dr. Whewell's Tidal Chart to be erroneous.

The Admiralty have from time to time sent out expeditions for the express purpose of extending our knowledge of oceanic phenomena, and the reputation of our Navy for intelligence is therefore involved in the question as to the course of the tide waves round the Great Southern Ocean.

9th May, 1906.

My Lords,—In a letter dated 30th January (acknowledged by note CN/2352) I claimed your Lordships' consideration for the question as to the direction of the main tide waves round the Great Southern Ocean, my justification for so doing being the fact that since (in the year 1868) I pointed out that their course is *eastwards* (instead of being, as generally supposed, *westwards*) the Admiralty have sent two important expeditions to report on oceanic phenomena in those regions.

I am quite sure the reputed leaders of science sincerely believed me to be mistaken in declaring those tide waves to have an eastward course, and were also just as sincere in rejecting as erroneous the theory of astral gravitation by which I was guided.

It does not appear to me to be unreasonable for me to appeal to your Lordships to bring the simple matter of fact to issue regardless of its bearing on any accepted theories. The phenomenon is one of the most important of the oceanic movements with which your expeditions had to deal, and it has just recently been forced again on my attention by a question in connection with the Steam Turbine which arose during a discussion of a Paper by the Hon. C. A. Parsons, read before the Institution of Civil Engineers.

It was stated that with a turbine doing 5000 revolutions per minute a loose ' bush ' placed between the rotating shaft and its case made 30 revolutions per minute backwards.

That, my Lords, is a simple illustration of gyroscopic action for which no explicitly defined explanation had been given until I applied to the gyroscope the same action of astral gravitation which had led me to the fact that the above-mentioned tide waves of the Great Southern Ocean move over the surface of the earth in the opposite direction to the motion of the sun and moon, by whose forces of gravitation they are raised.

The ' bush ' goes back, against the direct action of the rotating shaft, on the same principle as the tide flows against the direct action of the force by which it is raised. See Part II., pp. 119 to 121.

I do not ask your Lordships to theorise, but to place yourselves *above* theoretical considerations, by which I find that scientific societies (not excepting so practical an Institution as that of the Civil Engineers) are more or less biased.

21st October, 1906.

The success of the 'Dreadnought' *as a ship* entitles me to something better than the cold shoulder which the Admiralty have for thirty years persisted in presenting to me.

I have lifted that ship several feet farther from the rocks, the sand, or the mud below her ; given her several knots an hour more speed (allowing due credit to Parsons' turbine) ; made her a steadier fighting platform and a more commodious and comfortable residence than could have resulted under the principles of construction which have been exploded by the letters I have from time to time addressed to the Institution of Civil Engineers and to your Lordships. That is proved by considering those letters in connection with the views expressed at corresponding dates at the meetings of the Institution of Naval Architects, and I call on you, my Lords, to dissociate yourselves openly from the idea

that the inertia of the water does not resist a ship's way, as you have practically abandoned that wild delusion.

21st May, 1907.

My Lord,—In a letter dated 20th April, 1906, I endeavoured to point out how, in face of instructions by the Admiralty to various expeditions to high southern latitudes, the reputation of our Navy for intelligence is gradually becoming involved in the question as to the course of the tide waves round the Great Southern Ocean.

Except that your Lordship kindly acknowledged receipt of my letter, I do not know what has resulted from my appeal to the Admiralty to have the main point at issue determined.

The point then at issue is of vast importance as regards broad principles of Natural Philosophy, and of special importance for the elucidation of the course of oceanic circulation.

The recent wreck of the ' Suevic ' on the Lizard, followed two hours later by that of the ' Jebba ' on Bolt Tail, serves to give an illustration close at home of a revolving tidal action on a smaller scale, but dependent on the same principles as the question at issue in the Great Southern Ocean.

Those two steamships passed into the Channel from the Bay of Biscay, at the same time bound for Plymouth from the south. The ' Jebba ' passed in sight of Ushant on a course bearing a few miles east of the Eddystone. And the ' Suevic ' was farther west out of sight of Ushant, and steered direct for the Lizard to get a bearing from that point and then to pass west of the Eddystone.

Both vessels overran to about the same extent the distance reckoned by their respective logs and stranded during

thick fog, while their respective captains each supposed his ship to be many miles from the English coast.

At the time the vessels entered the Channel the Atlantic tide was also pouring in, and probably, I think, not far from its strongest current—creating action carrying them up Channel, and it seems clear that, before reaching the rocks, they must have been set back again down Channel by the falling tide.

The point these wrecks tend to illustrate is the view I have expounded to the effect that the tide enters the Channel with a cyclonic revolving motion which presses the under strata against the French coast and gives the surface a northward tendency from that coast; and also that there is a similar action of the same tide but on a greater scale as the tide passes between Ireland and Spain into the Bay of Biscay as well as the Channel. The greater depth of that bay gives the vertical action a greater proportion to the horizontal, and the pressure of the under strata against the north coast of Spain and west coast of France creates a surface flow northwards from the bay across the mouth of the Channel, which has become well known as Rennell's Current. That current, which at times runs with great force, is at other times not experienced at all, and these wrecks seem to indicate that the pressure of the approaching Atlantic tide immediately gives it an impulse northwards across the mouth of the Channel. It is part of the normal cyclonic motion with which the tide advances up Channel.

The action of astral gravitation which gives a revolving motion to all tide waves, was, when I first pointed it out more than forty years ago, considered by all leading scientific men so wild an idea as to place it outside the domain of reasonable discussion, and that attitude was all the more determined by my explanation of its incompatibility with the accepted definition of inertia.

Under the guidance of their erroneous idea of inertia, Sir George Stokes, William Froude, and others led our Naval Constructors to shape our battleships of the ' King Edward VII.' class and many other battleships and cruisers, and immediately I knew that practical application in shipbuilding was being deliberately given to that erroneous theory I called the attention of the Council of Civil Engineers to it in letters dated the 12th and 24th January and 21st October, 1903. On 21st December of the same year, in refutation of an argument regarding two specified shapes, I urged that ' the broader beam offers least resistance because the displacement is closer to the surface and therefore more easily effected,' and in letters of 22nd and 25th March, 1905, as a contribution to the discussion of a Paper which had just then been read by Lord Brassey, I gave plans and sections for preventing the excessively deep displacement of our battleships and cruisers. As my contribution was not allowed to appear in the ' Proceedings ' of the Institution I addressed the Lords of the Admiralty direct on the subject on 3rd May, 1905.

I now beg to urge on your Lordship's attention the fact that the same correction of the generally accepted theory of inertia which led me to find that the main tide waves of the Great Southern Ocean flow eastwards also led me to point out that up to the time I called attention to the subject, our Naval Constructors were giving too deep a displacement to our battleships and cruisers.

The building of the ' Dreadnought ' was not commenced for more than two years afterwards, and she has been so shaped that though about 2000 tons *heavier* than the ' King Edward VII.,' she draws about 2 feet *less*.

No clearly defined explanation for the superiority of her lines over those of any other battleship we now have in commission has been publicly given by any of our Naval

Constructors, and such explanation probably does not exist except in the letters I have addressed to the Institution of Civil Engineers to advocate such a change of principles as has been made, and in those I have since addressed to the Admiralty.

For the sake of the theory of astral gravitation which I published in the year 1866, I ask you, my Lord, to have the facts as regards the Great Southern Ocean tide established, and to give credit to that theory for having indicated the error in the principles of shipbuilding which prevailed before those under which the 'Dreadnought' was constructed were adopted.

Do not, my Lord, allow our Navy to appear to remain officially blind to the development of scientific knowledge in the theory of shipbuilding and in that of oceanic circulation.

31st May, 1907.

I venture to supplement my letter of 21st instant as regards the action of the flood tide up Channel from the Atlantic.

In the above-mentioned letter I showed that Rennell's Current across the mouth of the Channel would explain the stranding of the 'Jebba' and of the 'Suevic' on the night of 17th–18th March; and pointed out that those wrecks therefore tend to show that the said current is the northward roll of the tide as it approaches the Bay of Biscay and the Channel.

I find that Captain Beechey's Charts show that the northward motion setting on the English coast from the Bay of Biscay was certain to exist at the time those vessels entered the Channel; and I now beg to be allowed to place before Your Lordships and colleagues, his Majesty's Most Honourable Lords Commissioners, evidence to the effect that the

surface of the water in the flood tide has a similar lateral roll northwards as it advances up the Channel.

First: The Royal Mail Steamship 'Tyne,' steaming up Channel for Southampton, passed Portland Lights about 2 o'clock on the morning of 13th January, 1857, and about two hours afterwards struck the rocks of Kimmeredge Ledge in pitch darkness, and a rapidly rising tide which soon lifted her over that ledge of rocks to the mainland under St. Alban's Head. I cannot doubt the northward roll of the rising tide to have been the cause of that disaster; but the normal velocity of northward motion was then probably increased by a south-easterly gale which was blowing at the time and created a long, steady swell sweeping shorewards.

Secondly: The steamship 'Mohegan' bound down Channel on 14th October, 1898, passed Prawle Point at 2 hr. 25 m. P.M., which was about $2\frac{1}{2}$ hours before High Water at that Point; and she would therefore get the northward roll of the rising tide setting her into Plymouth Bay for some time after that; with the result that she struck the Manacle Rocks in Falmouth Bay instead of passing south of Lizard Head.

And *thirdly:* the steamship 'Paris' bound down Channel from Cherbourg intending to pass three miles south of the Lizard, struck the Manacle Rocks at 1.20 A.M. on 21st May, 1899. That was about 50 minutes after High Water at Ushant. She must therefore after getting her last bearings, at the Casquets, have felt the northward roll of the Flood Tide for a long time before she struck the rocks.

Those five wrecks combine to show that the Rennell Current is merely a development, on a greater scale, of a cyclonic motion of the advancing tide which exists also far up the Channel tending to give a surface motion of the water from the French to the English coast in the same manner as,

in the deeper water of the Bay of Biscay, the cyclonic action creates a stronger surface flow from the south of that bay towards Ireland and the coast of Cornwall.

Each of those five vessels was in the flood tide after obtaining her last bearings before striking the shore in foggy weather; and I venture to suggest that in combination with the facts already recorded on Captain Beechey's Charts they make it expedient for a warning to be given in the Admiralty Tide Tables to the effect that the Flood Tide from the Atlantic may generally be expected to have a tendency to carry a ship towards the Irish and English coasts instead of straight up Channel; and, with southerly winds, that lateral surface flow of the water may exceed four knots an hour.

24th June, 1907.

I most earnestly urge you not to rest satisfied with merely acknowledging receipt of my letters of 21st and 31st May: I am appealing to Your Lordship to exert the influence of your high position to put an end to a great injustice which is injurious to the best interests of the country. Even the mass of the scientific world will become thankful if your lordship (disregarding the worship of an effete fetish) forces fair consideration of the conflicting action of astral and terrestrial gravitation which has solved the problem of oceanic circulation and reformed the principles of ship-building by correcting erroneous ideas of resistance to the motion of ships.

Papers just read before the Institution of Civil Engineers, one on 19th instant by Dr. Elgar on 'Some Unsolved Problems in the Design and Propulsion of Ships'; and another on 21st instant by Professor Welch on 'High Speed Vessels'; both leave the epoch-making change to the design of the 'Dreadnought' untouched. Those Papers thus indirectly

confirm the statement in my above-mentioned letters to the effect that the change to the design of the 'Dreadnought' as a sea-going ship has not been explained anywhere except in the letters I have addressed to the Admiralty and to the Institution of Civil Engineers.[1]

I beg Your Lordship's attention to the following simple method of showing the practical difference between the theoretical views at issue :—

A stream of water just sufficiently rapid to move a pebble weighing one pound must be given double that velocity against the pebble to move a similar pebble four times larger. That is because the force of the stream is as the square of its velocity ; and, therefore, with double velocity it is able to do the four times greater amount of work required to move the four-pound pebble.

Double the *surface* velocity of the stream would not give that ratio, because the *mean* distance of the large pebble from the effect of friction at the bottom of the stream is greater than that of the small pebble. But, subject to a correction corresponding to that just stated, the force required to give a speed of 10 knots to a vessel of 4000 tons must be doubled in order to give that same speed to a vessel of 16,000 tons.

Thus, excepting the correction just indicated, only half the amount of force has to be expended to move a vessel of 16,000 tons as is required to move at the same speed four vessels of 4000 tons each, of the same relative dimensions.

The large ship has a deeper displacement than the smaller ships ; and the question at issue between the views I have submitted to Your Lordship and those by which our Naval Constructors had previously been guided, is as to the effect of the deeper displacement. My contention has been that it causes *increased resistance* as explained in

[1] See page 137.

my letter of 3rd May, 1905, to the Admiralty, and more fully in letters previously addressed to the Institution of Civil Engineers; whereas, according to the views of Sir George Gabriel Stokes, William Froude, and other Naval Constructors up to the time of the building of the 'King Edward VII.,' that deeper displacement *reduces the resistance.*[1]

The views under which the ships preceding the 'Dreadnought' were constructed are ably stated by Dr. Elgar in a Paper read before the Institute of Naval Architects on 11th July, 1893. Dr. Elgar says: 'Deep draught of water is a most important element of speed at sea, and is now strictly limited by the depth of water in the ports and docks used by the fast passenger steamers on both sides of the Atlantic. If the draught were not restricted . . . the resistance could be reduced by giving the section such a form and proportions as would increase the draught of water.'[2]

That view of the question is arrived at by mathematical argument based on the supposition of the so-called First Law of Motion being true to Nature; but the superiority of the 'Dreadnought' over the 'King Edward VII.'—arrived at by *increasing the beam in greater proportion than the draught*—constitutes a practical refutation of those views and of the so-called Law on which they are based.

I beg you, my Lord, to lead the Admiralty to recognise openly that astral gravitation does not allow that Law to assist a ship in its motion through the water, or a planet in its motion along its orbit.

[1] *September*, 1909.—It seems clear to me that the above argument as given in my letter of 21st December, 1903 (see pp. 144–145), in connection with the diagram reproduced in the preface of Part IV., together with other arguments in my letters to the Institution of Civil Engineers and to the Admiralty, led to the change of shape from that of the 'King Edward VII.' to that of the 'Dreadnought.'

[2] Dr. Elgar represented the generally accepted opinion on the subject at the time my letter of 12th January, 1903, was written. See page 137.

10th July, 1907.

Just for a moment please

> 'Awake to Science! leave all meaner things
> To low Ambition and the Pride of Kings.'

Many times in recent years our Navy, over which Your Lordship has the glory of presiding, has commissioned ships for the purpose of obtaining scientific knowledge in connection with oceanic phenomena.

As a result of the Admiralty expeditions of the 'Lightning' in 1868 and the 'Porcupine' in 1869, Dr. W. B. Carpenter, who represented our leading scientific societies on board those ships, read a Paper before the Royal Geographical Society in 1871 in which he declared that there existed a meridional circulation between the equator and each pole of the character shown in *figure* 1 of the diagram, p. 261, which he considered to be a consequence of the cold surface water sinking in the polar regions and rising to the surface in the equatorial regions in consequence of its variations in specific gravity.

Figure 2 shows the circulation in the same locality as given by sections which I published in the year 1866 on the supposition of the ocean being circulated by a conflicting action of astral and terrestrial gravitation.

Dr. Carpenter's idea as to the facts of the circulation was very widely accepted in the scientific world until after the return of the 'Challenger' Expedition in 1876, when Admiral Evans, then Hydrographer to the Admiralty, declared it had become evident that, all round the Antarctic regions of the Great Southern Ocean, there is a great bodily movement of the surface water northwards. That was a thorough confirmation of figure 2, and a direct refutation of figure 1 as shown on page 261.

The fact is, I knew quite well in the year 1866 from the records of Sir James Ross and Admiral Wilkes that my

VERTICAL SECTION OF THE INDIAN OCEAN

A. The Equator.
B. The Antarctic Regions.

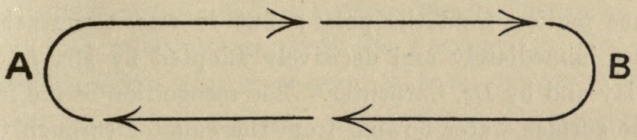


Fig. 1.

According to the Royal Geographical Society in 1871 and again in June, 1907.

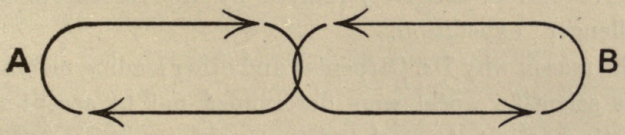


Fig. 2.

According to Astral Gravitation in 1866.

This is applicable to each of the oceans, in those of the Northern Hemisphere B representing the Arctic regions. For special explanation see page 260.

sections were correct for the Antarctic regions; and also that the records of Captain Scoresby and Captain Parry of their numerous expeditions in the Arctic regions showed a tendency to a similar course of circulation to be apparent there also.

The surface motion from the equator, as it flows against the trade winds in the central parts of each of the great oceans, I had considered to be much more at variance with generally accepted ideas of oceanic phenomena than the surface motion from the poles; but it was, nevertheless almost immediately and decisively adopted by Mr. A. G. Findlay and by Dr. Carpenter. The recognition of the fact of the surface water flowing from the equator through the central parts of each ocean made it impossible for Dr. Carpenter to suppose the winds to be the main cause of oceanic circulation; and he thought he had found the true cause in his explanations of the action of differences of specific gravity. His views on that point were, however, as above stated, decisively refuted by the records of the 'Challenger' expedition.

The reason why Dr. Carpenter and other leading members of the scientific world were determined not to accept the conflicting action of astral and terrestrial gravitation which had indicated the tendency of the surface water to converge towards the temperate zones, was because the admission of any such action of astral gravitation is incompatible with the idea of the so-called First Law of Motion representing a true principle in natural philosophy. The 'Astronomical Register' fairly represented the scientific world by tersely replying to my views in the words *Nolumus leges motus mutari.*

The single and simple point at issue is that Galileo, Descartes, and Newton supposed that the inertia of a body when set in motion tends to make it persist in that motion;

whereas I say that the momentum of a body in motion is resisted by its own inertia as well as by extraneous forces ; so that, neither on the earth nor in free space, can motion be continuously maintained without continuous expenditure of force for its maintenance, and, therefore, the momentum becomes exhausted as soon as motion proportioned to any impulsive force has been effected.

I lay the above statement before you, my Lord, because a few years ago a eulogy of the learning and the skill of our naval architects given in a Lecture which I heard at the Royal Institution awoke me to the startling fact that the so-called law was being given practical application in shaping our battleships and cruisers. I looked into the matter and found that it was attempted to give as deep a displacement as compatible with stability, under the idea that the inertia of the water does not resist a ship's way ; and that, therefore, a fish swimming deep enough to be out of reach of ' wave-making resistance ' might go on moving for ever under the action of a single impulse if it had not to overcome merely the effect of friction.

I had then for some time been endeavouring to induce the Council of the Institution of Civil Engineers to help me, or at least to obtain a fair hearing for my views regarding that so-called law ; and I brought this shipbuilding question into the argument considering that the already existing correspondence, together with the fact of Sir William White being a member of the Council, made that appear the most favourable course.

The success of the change from the shape of the ' King Edward VII.' to that of the ' Dreadnought ' is a thorough confirmation of the arguments I submitted to the Council of the Institution of Civil Engineers before the building of such a ship as the last named was dreamed of.

By sending out the ' Challenger ' the Admiralty obtained

practical evidence to the effect that the course of oceanic circulation indicated by the action of astral gravitation is that which in fact exists in the ocean ; and by building the ' Dreadnought ' the Admiralty have also obtained practical evidence to the effect that the principles under which all the other battleships and cruisers we now have in commission were constructed are erroneous.

His Majesty's Lords Commissioners have in fact in two directions established evidence which constitutes a deathblow to the fetish which has been paraded as the First Law of Motion ; and I beg you, my Lord, not to remain blind or impassive to the great scientific work which has been achieved by the Department of his Majesty's Government over which your Lordship is at present presiding.

19th July, 1907.

I was a few days ago informed that the Japanese Navy are making investigations with Deep Sea thermometers which they consider superior to any which have been used on board his Majesty's ships, and that the interpretation I put on the records obtained by her late Majesty's ship ' Challenger ' as explained in an essay on ocean currents which I published in the year 1900, is thoroughly confirmed by their researches. I am further informed that the Japanese authorities will neither just at present publish their records not allow any details to be given for publication.

The explanation which I gave, as above mentioned, in the year 1900 is merely an amplification of arguments which I addressed to her late Majesty's Lords Commissioners of the Admiralty in letters dated 8th November, 1887, 29th June, 1888, 14th November, 1889, and 14th July 1890.

With all due deference I now, in the first place, submit that the Lords Commissioners of those years were deplorably

misled in under-estimating the importance of the question for which I then endeavoured to obtain their consideration; and, secondly, that it is evident the Japanese Admiralty may now at any time publish the result of their explorations.

As the question now stands, the record of their researches would constitute the first official declaration of facts of oceanic circulation which will mark an epoch in the history of scientific knowledge. It would be out of place for anyone to grudge the Japanese the prestige which would redound to them throughout the scientific world, and more especially in the Asiatic world by becoming virtually the leaders of science in the work of oceanic research if that work had been originated by their scientific men and then tested and proved correct by their navy. In fact, however, on both those points the work has long ago been completed in this country. The result of the researches by her late Majesty's ship 'Challenger,' completed in 1876, gave a practical proof of the correctness of views which I had placed before the Council of the Royal Geographical Society ten years previously, and it is merely through a misunderstanding on the part of the scientific staff of the 'Challenger' as to the working of their thermometers that the facts now in course of being discovered by the Japanese Navy were not announced immediately after the return of the 'Challenger.' The Japanese researches are evidently justifying the suggestion in my above-mentioned Essay of 1900 that the scientific staff of the 'Challenger' are 'ignoring what will perhaps hereafter be considered one of the grandest results of the explorations.'

I call on you, my Lord, to rouse your colleagues, his Majesty's most honourable Lords Commissioners, jointly with your Lordship's own self, to take steps for reaping the prestige which is resulting from work done and expenditure incurred by this country before it is appropriated

by others whose justification will be its disregard or misappreciation by the rightful owners.

3rd August, 1907.

In a letter dated 19th July I pointed out how by the investigations the Japanese Navy are making with Deep Sea thermometers they stand at present to become discoverers of phenomena which a misunderstanding of the action of the thermometers used on board her late Majesty's ship 'Challenger' prevented the scientific staff of that expedition from recording. Unless the Lords of the Admiralty wake up to the fact that their ships have already made those discoveries, it will be useless after the announcement of the discoveries by the Japanese Navy to claim that a single scientific man in London had already announced them as having been made by the 'Challenger.' As a nation the Japanese will have the glory of discoverers, though they are in fact merely settling a question which has long been at issue, and are exploring under the guidance of scientific views originated in this country.

In connection with the foregoing it seems to me that forty years' consideration I have given to the subject of oceanic circulation imposes on me the duty of submitting to your Lordship reasons which make it expedient that the Admiralty should as quickly as possible despatch an expedition to the Arctic regions.

In letters addressed to the Royal Geographical Society on 27th August, 9th September, 14th September, and 14th November, 1902, I suggested a plan for a continuous effort by our Navy to reach the Pole by the West Greenland route.

Just at this moment that route may be considered to be in Commander Peary's possession; but, for all that is now known to the contrary, it is quite possible that an

easier route may be found by pushing north-eastwards from Bering Strait, keeping to the eastward of the position in which the 'Jeannette' entered the pack.[1]

I urged the above on the Council of the Royal Geographical Society on 7th July, but am informed they think it 'hopeless to expect our Government to send out either an Arctic or Antarctic expedition for purely scientific purposes.' I therefore urge direct on you, my Lord, the following considerations.

It is evident to me that Peary's recent expedition confirms my view as to the similarity of the circulation in the Arctic and Antarctic regions where not interfered with by land; and it is now well known that in the Antarctic regions every southern summer a fringe of ice breaks away from its position in the far south and spreads northwards with the surface water. As the parallels of latitude it traverses lengthen, it ceases to be a compact circle of ice; and experts now have some knowledge as to the position in which a p[illegible]hrough the ice circle may most easily be made for t[illegible]pose of reaching the wide expanse of open ocean [illegible]f the iceflow.

Commander Peary's information from the Arctic regions during the last four days of his daring venture northwards is:

On 18th April cracks and narrow leads increased and were nearly all active.

On 20th April we came into a region of open leads, trending north and south and the ice motion more pronounced.

And on 21st April, in 87° 6′ N., in this last spurt we crossed fourteen cracks and narrow leads, mostly in motion.

The great difference between the Arctic and the Antarctic regions is that in the Southern Hemisphere the ice moves away freely from the Pole, but in the Northern

[1] See footnote on page 271.

Hemisphere it has not that free course, and, therefore, packs against the coast lines by which it is obstructed. Peary's record for the far north, above quoted, shows, however, a tendency of the ice to break and spread in fragments in the same manner as the Antarctic iceflow, and seems to indicate the existence of open water beyond an ice circle similar to, but more dense than, the fringe which encircles the open water of the Antarctic regions during the southern summer.

Peary was more nearly north of the outflow between Greenland and Spitzbergen than either Nansen or Abruzzi in their expeditions, and it seems to me to be merely for that reason that he met with wider lanes of open water than they did ; but both of those expeditions were thwarted in the same manner by constantly recurring lanes of open water. All their records combine to falsify the idea long entertained by many Arctic experts to the effect that sledging would become easier in proportion as higher latitudes were reached. The expanse of open water two miles wide encountered by Peary in 84° N. combines with similar wide expanses which have been recorded near the north-west coast of Greenland leading northwards from Robeson Channel to show it to have been merely by an unfortunate chance that Captain Nares was forced westwards on the coast of Grant Land instead of finding a clear passage a long way northwards from the head of Robeson Channel.

It is now more than ever desirable that our sailors should be allowed a chance of making their way to the Pole by the West Greenland route under the plan suggested in my letters to the Royal Geographical Society above mentioned ; but as Peary is now working on that route, there is the Bering Strait route to fall back on, and for aught we yet know it may prove the easiest route.

Captain Scott has recently gained valuable experience in Polar work, and even if not successful by way of Bering

Strait, would be enabled to keep himself and a sturdy crew in readiness for other attempts.

In face of the information we now possess it seems a mistake to suppose it likely that the Pole can be reached by mere sledging during the summer months under Peary's recent plan on the supposition of the unknown area being water with a compactly frozen surface. If there is land west and north-west of Peary's last position, as seems far from improbable, his sledges might reach the Pole by its assistance; but otherwise ships or boats seem to offer the only chance along the surface of the earth.[1] The old sledgers' idea of reaching a smooth snow-covered ice-cap in the far north over which sledges might be pulled as easily as along the streets of St. Petersburg must now be considered a vanished dream. Perhaps an expedition organised as Captain Parry's of 1827, if started by Peary's route, might have succeeded; but it seems now evident that steamships could start such a boat-sledge expedition from much farther north, even if not able themselves to push on to the Pole.

I do not ignore the fact that Nansen, Abruzzi, and Peary each consider that with more good fortune as regards weather, or improved arrangements, their dog-sledges would have enabled them to reach the Pole. The idea of Arctic circulation by which they are guided is quite in accordance with what I forty years ago considered very likely to exist. It then seemed to me that the large influx of Atlantic water overwhelmed the normal circulation in the Arctic Ocean; and that the warm undercurrent would rise to the

[1] *September*, 1909.—The newly discovered land stretching northwards from 84° N. in about 100° W. keeps the ice from spreading away from the Pole so easily as it otherwise would. Commander Peary, looking 'north-west' from Cape Colgate on 24th June, 1906, saw land; and four days later again saw the same land from Cape Hubbard. Dr. Cook, in his northward journey from the latter Cape in April 1908, passed nearer to that land and reports that it extends a considerable distance northwards.

surface in the direction of Wrangel Land and thus create open water there every summer instead of at the Pole.

The importance of any such derangement of the normal circulation was, however, shown by the drift of the 'Jeannette' to be much less than I had supposed possible; and Nansen's subsequent drift in the 'Fram' led me to regard it as a matter of comparatively trivial importance, and seemed to me to make it evident that the undercurrent rises to the surface at the Pole, or as near to it as any land that may exist there allows it to approach. That is where the open water is to be looked for during the summer months. It doubtless even in summer may get coated with thin ice which before it has time to get thick enough to be safe, except in winter, becomes broken up by tidal pressure or wind pressure. Sir William Parry's boat-sledges would, to say the least, be a *safer* means of transit in such a region than Commander Peary's dog-sledges.

What I contend for, however, is that though any vessel commissioned by His Majesty's Government might be supplied with both boat and dog sledges, the possibility of navigating the steamship herself to the Pole ought not yet to be abandoned; and our sailors should continuously be allowed a fair chance of gaining the prize to which they were the first to aspire. Her late Majesty Queen Victoria gave the order to go to the Pole: will you not, my Lord, together with his present Majesty's Lords Commissioners of the Admiralty, make some effort to have that order obeyed? Officers and men are ready to comply if not held back by you. In the year 1849 Captain Kellett and Lieutenant Moore cruised to the north-east of Bering Strait in H.M.S. 'Herald' and brig 'Plover,' and their report of the loose, drifting ice-fields seems to make it possible that a determined attempt to break through northwards in that region might be successful. Their duty was,

however, to search along the shore for information of the Franklin expedition, not to find a passage northwards.

I beg to impress on your Lordship the fact that not long ago the 'North-East Passage'—from the Atlantic to the Pacific—was supposed to be blocked by impassable ice in the Kara Sea; but it is now known that every year the ice moves away from the west coast of Yalmal, leaving a clear seaway northwards. It seems almost obvious, in face of records we now have from the north-west of Greenland, that the same action of forces which every year opens a route northwards from the west coast of Yalmal also opens a similar route off the north-west coast of Greenland; and that similar alternations of ice pressure, eastwards and westwards, may not unreasonably be expected to exist off whatever coast lines there may be to the north-east of Bering Strait.[1]

At any rate that region seems worthy of the attention of our sailors as an alternative to the West Greenland route.

11th December, 1908.

Sir,—Under dates of 21st and 31st May, 1907, I addressed letters to your predecessor in office on the subject of the Channel Tides, showing how a hitherto unrecognised theoretical action appears to have been the cause of various shipwrecks which have occurred notwithstanding the vessels being in charge of well experienced captains or pilots.

I now address your office again on the subject in consequence of having received a Report published a few months ago by the Government Office of Public Works of the Argentine Republic on the hydrography of the River

[1] *September*, 1909.—Roald Amundsen has arranged to explore that route next year in Nansen's brave old ship the 'Fram.'

Plate, which has an important bearing on the views I have submitted to your office on the Channel Tides.

The two sketches annexed, p. 273, show the points for which I beg your attention.

The first sketch shows that the tide from the North Atlantic entering the English Channel is so much stronger on the French than on the English coast that it crosses the head of the Channel and passes Beachy Head, returning down Channel, before the tide along the English coast reaches Beachy Head.

And the second sketch shows that the tide from the South Atlantic entering the River Plate is so much stronger on the Argentine than on the Uruguay coast that it crosses the head of the River and passes Martin Garcia, returning down the River before the tide along the Uruguay coast reaches Martin Garcia.

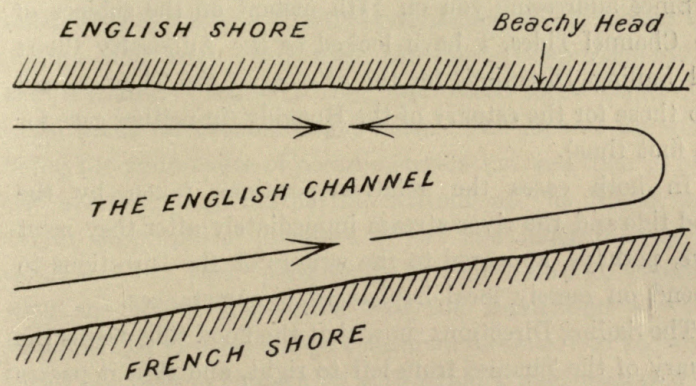
As cyclones run with the hands of a watch in the Southern Hemisphere and against them in the Northern Hemisphere, both those localities show the tendency to cyclonic action.

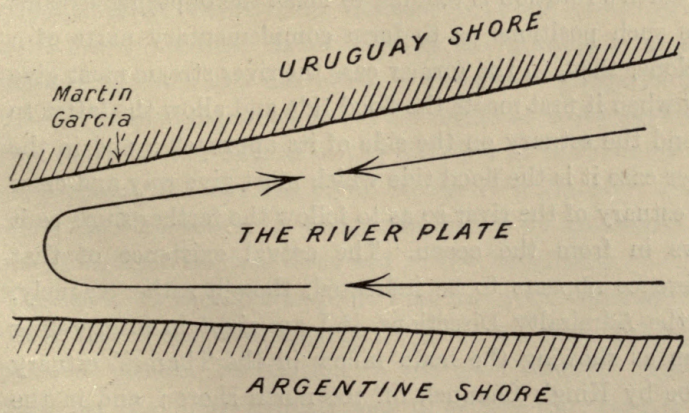
The corkscrew motion with which the tide advances up the estuary, causing the surface of the stronger branch of the tide to press away from the coast and thus to impede the advance of the tide on the opposite coast, has not been recorded in the River Plate Report. The water is so shallow in the latter estuary that the surface roll will probably not be so clearly defined as it appears to me to be in the English Channel; but I have no doubt that some indication will appear in the course of further investigations.

That is a small but important feature in the action of astral gravitation which in the year 1866 I indicated as being the cause of the main features of oceanic and atmospheric circulation. It has become clear (as I have shown in previous letters) that the leaders of the expeditions organised by your predecessors in office were mistaken in the various

DIAGRAMS ILLUSTRATING THE SAME TENDENCY TO TIDAL CYCLONIC ACTION IN THE RIVER PLATE AS IN THE ENGLISH CHANNEL

For Special Explanation see Letter of 11th December, 1908, page 271





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rival explanations in which their own records were misinterpreted ; and it is detrimental to the highest interests of this country for His Majesty's Lords Commissioners to persist in passive resistance to the results of their own explorations.

28th December, 1908.

Since addressing you on 11th instant on the subject of the Channel Tides, I have looked at the Admiralty Chart and Sailing Directions for the estuary of the Thames, and also those for the estuary of the Hooghly (in neither case for the first time).

In both cases the relative positions taken by the flood tide and the river stream immediately after they meet have, perhaps, appeared to the writers of the Directions to depend on merely local haphazard circumstances.

The Sailing Directions show that the flood tide passes the estuary of the Thames from left to right, and that it passes the estuary of the Hooghly from right to left. Therefore, if the earth's rotation is tending to make the opposing streams take such positions as to form complementary parts of a Cyclone, then, in the former case the river stream must give way when it first meets the flood tide and allow the latter to ascend the estuary on the side of its approach ; and in the latter case it is the flood tide which must give way and cross the estuary of the river so as to follow the farther shore as it flows in from the ocean. The actual existence of that difference appears to be indicated, though rather vaguely, by the Admiralty Directions, if I am right in interpreting them as showing the main inflow in the Thames' estuary to be by King's Channel, off the north shore ; and in the Hooghly estuary by channels which pass not far from Sangor Island on the east shore. In the former case the river stream will probably be found to run for some time

on the *far* shore while the tidal stream is ascending along the *near* shore ; and in the latter case those conditions will be reversed, though apart from the action of the earth's rotation there seems no reason why the tidal stream should not ascend the Hooghly estuary by the near shore as it does in the Thames.

It is now forty-two years since I published the broad outlines of the action of astral gravitation in circulating the ocean ; and forty years since the Lords Commissioners of the Admiralty began fitting out their most important expeditions for oceanic exploration. The idea of the stars being the main cause of oceanic circulation was then so new and startling that the hostile attitude of The Royal, and Royal Geographical Societies in the first instance may be excused ; but their persistence in that attitude, together with the Royal Institution and the Institution of Civil Engineers, is now becoming a national disaster. If the duty to see fair play in connection with explorations organised by his Majesty's Lords Commissioners does not, under all circumstances, devolve on themselves, their intervention is at least expedient under existing circumstances, as the manner in which fair discussion is thwarted as regards the bearing of their explorations on the action of astral gravitation must be detrimental to the interests of the country.

A recent instance was given on the 17th January, 1905, when the obstruction to navigation caused by sandbanks in the Hooghly river was discussed in the Institution of Civil Engineers. I then pointed out that the worst obstructions occur where the tidal current is prevented from taking a cyclonic position in relation to the river stream. And I must say that I am scarcely able to believe the evidence of my own senses as they make me record that, not only did the Council of the Institution refuse to allow my argument to be recorded in the Proceedings, but they also subsequently

obtained from a rival theorist, and published in the Proceedings, an argument based on the idea that the momentum of the streams tends to make them describe straight lines along the surface of the earth, notwithstanding the fact of that surface being in motion as the water moves over it; and that stands to-day officially recorded as the opinion held in the discussion unchallenged by any dissentient voice, notwithstanding explicit protests which I have submitted.

The perversity of the Council of the Institution of Civil Engineers forces me to appeal to his Majesty's Lords Commissioners of the Admiralty to keep their tidal charts of our coasts in the position of the leading authority; and as the matter has a broader bearing on higher interests of the country, it further calls for the attention of his Majesty's Government.

It would be out of place for me to address them on the basis of the Admiralty explorations except through your office, and, therefore, in order to facilitate your action, I enclose a duplicate of this letter and also of my letter of 11th instant, above referred to, asking you to place them before his Majesty's First Lord of the Treasury.

14th January, 1909.

My Lords,—I beg leave to acknowledge receipt of your Lordship's collective reply to the letters which I addressed to the First Lord on 11th and 28th December.

The work of oceanic research has through many centuries been so well appreciated and actively conducted by our Admiralty as to have made this country pre-eminently renowned as benefactors to all commercial communities; and in the interests of that work it seems expedient for me to press on your Lordships' attention some points connected with it.

When I first consulted the Admiralty, and other publications, for the purpose of finding evidence in support of my view to the effect that the seas surrounding our shores are each subject to cyclonic circulation, it was in great part only inferentially that I could connect various recorded facts so as to be able to declare that generalisation to be verified. The recent publications by the Admiralty, however, describe the circulation in the North Sea, in the English Channel, and in the Irish Sea in such a manner as to make it impossible for common-sense to refrain from recognising that my above theoretical generalisation has in the meantime supplied a clue which has enabled the experts employed by the Admiralty to trace the true connection between otherwise apparently disconnected facts dependent each on its own local haphazard circumstances.

The work is, however, far from complete ; and chaos will appear to prevail in recorded facts in some localities until a further theoretical clue is employed for its unravelling.

I some years ago pointed out that between the neighbourhood of Dieppe and our shores a direct local action of the moon makes itself felt, interfering with the normal course of the main derivative tide which enters the Channel from the Atlantic. In that same manner the tide which enters the Irish Sea from the north of Ireland and that which enters the North Sea from the north of Scotland must each be more or less affected by direct local action of the moon in those seas and in more northerly regions.

There can be no doubt that in the absence of direct local action the tidal flow from the English Channel to the North Sea through the Strait of Dover would considerably exceed the flow in the opposite direction through that strait. And also the flow from the Irish Sea to the Atlantic between the shores of Scotland and Ireland would considerably exceed the flow in the opposite direction through that passage.

I do not find that any information as to the relative strength and duration of the opposite streams in those localities at present exists. That information is, however, necessary for a complete understanding of tidal action on our shores; and accurate information from those localities would also be of great value for the purpose of elucidating tidal action in all parts of the world.

1st September, 1909.

The extract from the log of the steamship 'Clan Macintyre' published this morning shows that on the night of 27th July the 'Waratah' while in the stream of the Agulhas current must have met the heavy westerly gale recorded by the 'Clan Macintyre.'

If the vessel were disabled by the heavy seas caused by the gale blowing almost dead against the current, she would drift with the current which sometimes forms one of the most rapid to be found anywhere in the ocean setting from the position where she would have met the gale across the track of vessels bound round the Cape. That would carry her into a region south of any 'beaten tracks' until stopped by the cold surface flow from the south under which the Agulhas current disappears. There she might be held for a long time before drifting back to lower latitudes.

As no wreckage from her has been met with by any of the search parties, I venture to suggest that one of our ships of war might be sent from Cape Town to follow the Agulhas current wherever it goes until it disappears under the cold flow from the south.[1]

[1] 24*th September*, 1909.—A long way south-west from about 32 S., 29 E., where the 'Waratah' was last seen, there is a considerable area of warm water, usually, if not always, disconnected by cold surface water from any surface flow of warm water. I believe it travels as an undercurrent south-eastwards from the middle of the South Atlantic, but perhaps the momentum of the Agulhas current sometimes carries it further in that direction than is generally supposed.

A cruise along the line of junction of the warm and the cold streams would form a valuable record, even if not successful in the main object.

I have the honour to be, my Lords,
Your most obedient, humble Servant,
WM. LEIGHTON JORDAN.

The Secretary of the Admiralty begs to acknowledge the receipt of Mr. W. L. Jordan's letter dated 1st instant on the subject of the 'Waratah,' which will receive attention.

Admiralty, 2nd September, 1909.

PART V

LETTERS IN CONNECTION WITH THE ROYAL INSTITUTION THE INSTITUTION OF CIVIL ENGINEERS AND THE ADMIRALTY

NOVEMBER 1909 TO JUNE 1910

TO

THE REV. WILLIAM BRAMLEY-MOORE, M.A.

AND

MRS. BRAMLEY-MOORE, ELLA BRADSHAW JORDAN

THIS VOLUME IS DEDICATED

IN TOKEN OF APPRECIATION OF THEIR KIND INTEREST

IN THE WORK THROUGH FORTY-FOUR YEARS

PREFACE TO PART V

FIGURE 2 in the Frontispiece (reproduced from Part IV) [1] shows the main feature of the meridional circulation of each of the great oceans; and if the water covered the whole earth with equal depth that circulation would exist all over the ocean, being in such case undisturbed by the obstructing action of coast lines. It results from the conflicting action of astral and terrestrial gravitation as explained in works which I published in the years 1866 and 1868.

Figure 1 represents the circulation which was declared by Dr. W. B. Carpenter to exist, and was attributed by him to the interaction of equatorial heat and polar cold as explained in a Paper read by him before the Royal Geographical Society on 9th January, 1871, based chiefly on explorations made by The Admiralty in the preceding three or four years. The further, and more extensive exploration by the Admiralty in the 'Challenger' Expedition resulted in a thorough refutation of Dr. Carpenter's modification of the Sections of Circulation which I had submitted to the Council of the Royal Geographical Society in 1866 as above mentioned, and established the correctness of the latter sections.

For many years the only opposition to my views regarding the circulation consisted in attempts to give other explanations for the facts than those which had led me to look for them when the existence of the general course shown in Figure 2 was not known.

[1] Now given on page 261 of this volume.

That had been the position of the question for many years when, on the 10th June, 1907, Dr. Otto Petterson read a Paper before the Royal Geographical Society evidently quite unconscious that he was reproducing the same theory as Dr. Carpenter had originated thirty-six years previously in the same Lecture Hall; and he was also unconscious that the circulation he treated as in fact existing had been definitely refuted thirty years previously by the 'Challenger' Expedition.

I will not reargue the questions of fact and theory involved nor conjecture any explanation as to the strange position in which The Royal Geographical Society has been placed in connection with them. Sooner or later it will have to be acknowledged that I correctly solved all the main features of the circulation in the Charts and Sections which I submitted to the Council of The Royal Geographical Society in the year 1866, but I have not thought it right to end 'The Sling Letters' without a statement to the foregoing effect, and I now leave them to speak for themselves as to the vaster extensions of scientific knowledge which have since been arrived at by extending to other matters the same theoretical action of forces which solved the question of oceanic circulation forty-four years ago.

Royal Societies Club,
24*th June*, 1910.

LETTERS

To Sir William Crookes, F.R.S., Secretary, the Royal Institution.

9th January, 1910

Dear Sir,—it would be out of place for me to endeavour to trespass on you by allowing myself the pleasure of indulging in any well-merited eulogies on the series of 'Juvenile Lectures' which Mr. Duddell concluded this afternoon;[1] it would, however, be equally out of place for me to refrain from the displeasurable task of submitting to the Royal Institution a protest against a mischievous error in the lecturer's teaching.

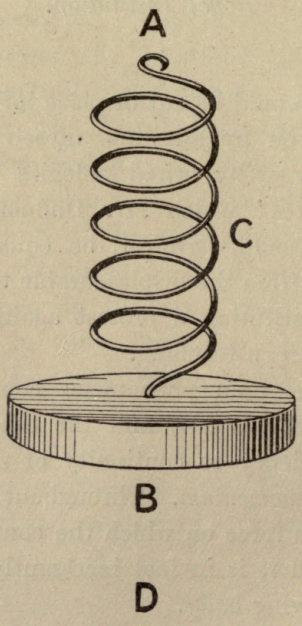
In explaining some phenomena connected with the Pendulum, the Fly Wheel, and the Spring, the lecturer ascribed the tendency to continuity of motion to *inertia*, instead of to *momentum*. Throughout that argument momentum is the force on which the continuity of motion is dependent, and it is in fact incessantly *opposed* by the inertia of the moving body.

Then, putting the weight at the end of the spring into a jar of water, the lecturer declared the reason for the retardation of the motion being quicker than in the air to be due to friction, ignoring any resistance due to the inertia of the water. The fact is that, as the resistance of inertia is equal to the mass to be moved, the resistance to

[1] Six Lectures on 'Modern Electricity,' by William Duddell, Esq., F.R.S., M.I.E.E., M.R.I., delivered 28th December, 1909, to 8th January, 1910.

the motion of any body is therefore greater when moving through water than when moving through air.

Just look at a diagram. From the point A let the weight B be suspended by the spring C in water. Then let the weight be pulled down to D and released. The recoil of the spring then, as it brings the weight up and causes



it to oscillate up and down for some time, has to do the work of displacing the water to make way for the motion of the weight. According to the lecturer's school of physics, the motion of the weight is resisted merely by the friction of the water, whereas I contend that the inertia of the water resists the motion of the weight.

The inertia of the water, which resists the motion imparted to the water by the moving weight, causes the

greater amount of retardation because the water has more mass than the air. The friction of the particles in contact with the weight is merely a part of the resistance. And, as I have shown in my 'Treatise on the Action of Vis Inertiæ in the Ocean,' published in the year 1868, inertia is the opposite aspect of universal gravitation, and friction the reciprocal action of gravitation between bodies in contact, all the resistance to the motion of the weight is, therefore, directly or indirectly due to inertia.

To sum up the points at issue as above indicated: the lecturer ascribed to inertia what is really due to momentum and is opposed by inertia; and he ascribed only to friction what is entirely due to inertia: friction being in all such cases a more or less important component of the resistance due to inertia.

The special reason which entitles the error on which I have above commented to the term mischievous has been so elaborately given in Part IV. of the series of letters to which the present appertains, that I will just now add nothing further beyond stating the fact that it is precisely that by which forty years ago Sir George Gabriel Stokes and William Froude began to be guided in shaping our battleships, and which was more or less acted on by our Naval Constructors until after the launching of the 'King Edward VII.'

It is deplorable that the Royal Institution should still be teaching that error six years after its practical abandonment by our Naval Constructors.

25*th January*.—The fact of the foregoing having remained in abeyance until to-day is recalled to my attention by a article in to-day's *Times*, which says of 'The introduction of the "Dreadnought" type of warship' that: 'it is full of technicalities little fitted for popular discussion'; the fact

is that by far the most important point which enabled the transition from the 'King Edward VII.' to the 'Dreadnought' type to be effected is the theoretical argument which showed that ships of heavier tonnage than the former might be built advantageously in many respects without any increase of draught; and there is nothing now to make public discussion of that point unsuitable except personal considerations affecting only such public officials as are conscious of having achieved reputation in borrowed plumes.

19th February, 1910.

'Alas that Scottish maid should sing the combat where her lover fell! Alas that Scottish bard should live the triumph of our foes to tell!'

The address delivered in our Lecture Hall last evening [1] by Professor H. H. Turner on the two comets now visiting us was representative of more than the Royal Institution—delivered by one of the ablest protagonists of the University of Oxford in an institution which has long been an appanage of the University of Cambridge, it was in fact representative of the scientific views now dominant in all our professional seats of learning, the Royal Society being to the front not only in the person of the lecturer, but also by the presence of many of their most renowned members.

The point of chief practical importance in the lecture seemed to me to be the two successive photographs of one of the comets taken on the same night. And in connection with that I wish to impress on you, Sir William Crookes, the fact that Professor Turner declared that the change in the tail which had occurred in the interval showed that it must be constituted of particles in process of being pro-

[1] Lecture on 'Halley's Comet,' by Professor H. H. Turner, D.Sc., D.C.L., F.R.S.

jected from the comet by the outward pressure of sunlight, or by some not yet understood electric action.

I really do not think there is anyone who appreciates more highly than I do the conquests of learning in the domain of science, but, nevertheless, with all the deference due to its reputed representatives, I protest against such a statement as the above being made in our Lecture Hall as being a long way behind what is the present state of knowledge.

My protest is not against anything said by Professor Turner, but against a glaring error of omission on his part. The transverse action of gravitation which endeavours to scatter the comet whenever it is anywhere near the plane of the sun's equator, just in the same manner as it plays pranks with our atmosphere each time we cross the plane of the sun's tropical regions, was ignored by Professor Turner.

When I published my first work in 1866 I knew nothing of that transverse action; but that work, nevertheless, by its elimination of Galileo's definition of inertia, cleared the way for its reception; and in a penny pamphlet which I published in Buenos Ayres in the year 1870 I gave a mathematical statement showing that transverse action to be a corollary to Newton's law of gravitation.

By ignoring that transverse action of gravitation in favour of such a triviality as the pressure of light, or any nondescript action of electricity, last night's lecture, notwithstanding all its fascinating points of interest, placed the Royal Institution, and with us also the Royal Society and our leading Universities, behind the true position of to-day's scientific knowledge.

26th February, 1910.

In last night's Lecture [1] Lord Rayleigh alluded to the sea-water off Algiers as being green, without giving any reason for the difference between it and the blue water of other parts of the Mediterranean which he visited.

There certainly are in the ocean, not only on coast lines but also on the surface of the deep sea, large areas of water intrinsically different in colour from that met with in some other large areas, and forty-five years of study of the subject of oceanic circulation has led me to the opinion that the water of the deep blue colour has been flowing for a long time on the surface of the ocean. That is the case with the surface flow from the Mediterranean into the North Atlantic, and also with much of the surface water it meets there flowing from the north.[2] I have, however, long ago pointed out that an undercurrent of water which sinks near the Banks of Newfoundland bifurcates against the coast of Africa outside the Strait of Gibraltar ; and the green water at Algiers confirms the idea that the outflow of the blue surface water is counterbalanced by water from that undercurrent, entering the Mediterranean partly as an undercurrent and partly by creeping along the shore on the south side of the Strait. From the Banks of Newfoundland to the Strait of Gibraltar the motion of the bottom water must on the average be very slow ; and among the vast array of facts which I have accumulated in connection with the ocean currents there are enough to make it appear that under those conditions the water loses its blue colour and reappears on the surface as green or brownish-green.

[1] Lecture on ' Colours of Sea and Sky,' by the Right Hon. Lord Rayleigh, O.M., P.C., D.C.L., LL.D., D.Sc., F.R.S., M.R.I., Hon. Professor of Natural Philosophy R.I.

[2] The flood tide from the Atlantic overpowers this circulation in the middle of the Strait, and makes the principal surface flow inwards.

5th March, 1910.

I attach so much importance to Lord Rayleigh's description of the manner in which the sea-water off the coast of Algiers differs in appearance from the general appearance of the sea-water in the Mediterranean that I consider it desirable for me to supplement my letter of 26th ult. on the subject.

What gives Lord Rayleigh's records importance is that I cannot see how it is possible to give a clearly defined explanation of the facts by any other theoretical action than that of the conflicting action of astral and terrestrial gravitation. His records, in fact, afford a suitable opportunity for broadly stating some points in elucidation of that conflicting action of gravitation.

1. As the earth turns eastwards on its axis the action of astral gravitation creates a surface flow westwards from the Mediterranean to the Atlantic, because the surface water is less firmly held to the earth than the lower strata of water. And the earth's gravitation restores the level of the sea as fast as it is disturbed by that outflow, by a counter-movement of the under strata, or of the coast water, eastwards.

2. That counter-current may not be so easily detected as the outflow, because it may be effected by the motion of a volume of water twenty or a hundred times greater than that of the surface outflow; so that if the outflow runs at from one to five miles an hour, the inflow may average only one-hundredth part of those velocities, and be clearly apparent only in some small areas.

3. That conflicting action of gravitation is a consequence of the inertia of the water which resists the effort of the earth's gravitation to move the ocean with the earth's rotation. And it refutes the idea originated by Galileo to the effect that a body in motion tends to maintain itself in motion in perpetuity without any renewed action of

motive force. That is the main point which is at issue in the question as to whether the circulation through the Strait of Gibraltar is, or is not, caused by a conflicting action of astral and terrestrial gravitation.

4. The transition from green to blue off the coast of Algiers may perhaps be very gradual; but in some parts of the great ocean it is abrupt.

In the voyage from the Cape Verde Islands to Pernambuco by the mail steamers any passenger who takes the salt-water bath every morning finds that for one or two days when near the equator the water is decidedly colder than farther north, and that it again becames warm farther south. The cold area often has a decidedly green tinge, clearly differing in colour from the blue and warmer water north and south of it; and a few hours before reaching the outer anchorage ground off Pernambuco the steamer sometimes passes quite abruptly from deep blue into pale green water. The fact is that the blue water has come all the way across the Atlantic as a surface current in the equatorial regions, and it bifurcates, not directly against the coast line of South America, but against a cushion of green water which rises from the under strata along the coast line. It seems to me impossible for the phenomena in that locality to be fully appreciated without forcing a conviction to the effect that any possible action of the winds, or of differences of specific gravity, are inefficient for the production of such massive movements of the ocean as are apparent in that, and in many other localities.

9th March, 1910.

When writing on 26th February and 5th instant, I had forgotten the most generally accepted facts of the circulation through the Strait of Gibraltar which I had before me when

I replied to Dr. W. B. Carpenter's views thirty-nine years ago. What was recalled to mind by Lord Rayleigh's record of green water off the coast of Algiers was the fact that on the only occasion on which I passed through that Strait, entering the Mediterranean, there was so strong a surface current outwards to the Atlantic that the steamer (a heavily laden cargo-boat) in which I was a passenger was for many hours able to make very slow progress against it.

I snatched at the green water of Algiers as indicating an inflow in the under strata to counterbalance the above-mentioned outflow on the surface. And it now appears to me that action of forces plays no unimportant part in the circulation between the Mediterranean and the ocean. There has, however, been a very generally received idea to the effect that the surface is constantly streaming inwards through the Strait to maintain the level, or to reduce the specific gravity of the Mediterranean; and when writing in the year 1871 I left Dr. Carpenter in complete possession of that Strait as an illustration of his theory of circulation, though it then appeared to me, as it does now, an open question as to which theory plays the most important part in that locality.

10th March, 1910.

Sir Archibald Geikie, in the work on 'Physical Science in the Time of Nero' just published by him conjointly with Professor John Clarke of Aberdeen, says: 'It behoves us to be on our guard lest, from our much higher standard of knowledge, we may be tempted to look with amused contempt on the puerile conceptions of nature to be met with in the writings of the ancients—the grave assertion of absurdities as actual facts, the inept analogies, the confident explanations which are no explanations at all, and the

complete absence of any attempt to test by examination or experiment the validity of statements which with but little trouble could have been disproved.'

As the writer of the above is President of the Royal Society, in which your scientific renown makes you one of the most responsible members, I ask you, as a member of that Society as well as Secretary to the Royal Institution, to consider as to whether either of those Societies is now giving reasonable heed to his timely exhortation.

Was Seneca more blindly led by opinions of Aristotle than they are by Galileo's explanation of the continuity of the earth's motion ?

In the year 1868 I pointed out that as a matter of fact the tide waves of the Temperate Zone in the South Indian Ocean flow *eastwards, meeting* the moon, not *westwards, following* the moon, as asserted by all publications of the Royal Society bearing on the question. The statement that the tide waves follow the moon round the Great Southern Ocean is one whose validity could with but little trouble be disproved.

How much more water will you allow to run under London Bridge before recognising that ' it behoves us to be on our guard against the grave assertion of absurdities as actual facts—confident explanations which are no explanations at all, and the absence of any attempt to test by examination or experiment the validity of statements which with but little trouble could be disproved ? '

16th March, 1910.

I have already (in my letter of 9th instant) said that in discussing the subject of oceanic circulation in the year 1871 I treated the generally accepted records of circulation through the Strait of Gibraltar as supporting completely

Dr. W. B. Carpenter's theory of circulation in that Strait, which there requires relative directions of the surface and under currents in the opposite directions respectively to those required by the conflicting action of astral and terrestrial gravitation: the latter requiring a surface current *outwards* and the former *inwards*.

Major Rennell suggested that the reason for the Gulf Stream turning eastwards from the coast of the United States might be the action of an indraught to the Mediterranean to supply the waste of water by evaporation in that sea; and ever since his time there has been a very generally received idea to the effect that the supposed excess of evaporation over the supply of water by rivers and rain does in fact exist in that sea, and is the cause of the indraught through the Strait.

The green water on the Algerian coast, of which Lord Rayleigh told us in his Friday Evening Lecture on 25th February, is so strong an indication of an inflow from the under strata of Atlantic water that I have turned to see what has been added to our knowledge of the subject by the Admiralty records since last I dealt with it.

I find that tidal observations taken on the shallow ridge just outside the Strait show that the tides flow over that ridge in opposite directions for equal periods not only on the surface but also in the under strata.

It is evident that, if those records are to be relied on as being even approximately correct, we must abandon the idea of evaporation in the Mediterranean being so greatly in excess of the supply of water by rivers and rain as has been supposed since the days of Major Rennell.

The evidence thus seems to indicate that in the narrow part of the Strait the surface flow of the tides is increased during the inflow by a tendency of the lighter Atlantic water (after crossing the outside ridge) to overrun the

Mediterranean water, as argued by Dr. W. B. Carpenter, and during the outflow by the normal drag of astral gravitation: in each case compensated by equivalent tendencies to counter-flow in the under strata, the Atlantic flow predominating on the surface and the Mediterranean flow below the surface in and near the Strait.

In confirmation of the above view there is corroboration now to be found in the Admiralty records.

Though the currents over the ridge outside the Strait are said to flow and reflow equally with tidal action on the surface as well as in the under strata, that certainly ceases to be the case through and immediately inside the Strait. The surface drag of astral gravitation tends to bring surface water in the deep parts of the sea from the east and north-east to the Strait, causing counter-currents eastwards along the shores, but the westward flow is to a great extent thwarted or submerged by a predominant inflow of surface water from the Atlantic through the Strait. The circulation of Atlantic water all through the Mediterranean is clearly due to the fact that the earth's rotation creates a tendency for the main course of the inflow to deviate from the direction from which the outflowing water comes to meet it, as I have pointed out at some length in letters to the Institution of Civil Engineers on the formation of sand banks in the Hooghly and in the Netravati rivers.

The water which enters with any tide through the Strait tends to make a cyclonic tour all round the Mediterranean before in its turn becoming part of an outflowing tide. It thus tends to create eastward currents on the south side of the sea, and westward currents on the north side; and that circulation prevents the Mediterranean water from continuously increasing its specific gravity in relation to Atlantic water. Some slight excess of evaporation in the Mediterranean seems to be shown by actual records of

specific gravity, and if so it must make the tidal flow east and west over the ridge outside the Strait not so equal as appears from the records to which I have alluded, though as a factor in the circulation its importance is not so great as supposed by Major Rennell.

The Admiralty records which corroborate the above view, initiated by Lord Rayleigh's record of green water at Algiers, are :

(1) Indications of such a flow from the north and east between Sardinia and the Balearic Islands as would result directly from the drag of astral gravitation.

(2) Eastward currents along the coast of Spain from the Strait as far as the Balearic Islands, forming such a counter-current as would result from westward pressure of the deep parts of the sea towards the Strait.

(3) The corresponding current eastwards off the coast of Africa is found in its proper place, but its magnitude is so increased by the surface inflow through the Strait as to make it the most important surface current in the Mediterranean Sea. It obliterates, at least on the surface of the sea near the Strait, a great part of the westward drag of astral gravitation,[1] and it can be traced eastwards off the coast of Africa to Syria ; then northwards ; then westwards between Rhode Island and Asia Minor ; and again westwards in the Gulf of Lyons ; it then probably joins the draught southwards and south-westwards above mentioned, as the counter-current eastwards created by the westward pressure of the deep parts of the sea prevails along the south-east coast of Spain.

19th March, 1910.

Sir J. J. Thomson in his Friday Evening Lecture on 'The Dynamics of a Golf Ball' yesterday gave us an

[1] The water of the Atlantic tide falls over the ridge outside the Strait, and must of necessity be for some distance a surface current.

interesting fragment in the history of the development of scientific opinion.

Lord Rayleigh in the year 1877 corrected what was then a very generally accepted opinion to the effect that the deviation of a spinning golf ball from the straight line of its projection is due to a frictional rolling of the ball on the condensed air in front of it; and Lord Rayleigh then substituted a new explanation to the effect that on the side of the ball on which the motion of the spin is in conjunction with the motion of projection the greater frictional resistance to the combination of the two motions causes the ball to deviate in the direction of least resistance.

Sir J. J. Thomson adopted Lord Rayleigh's explanation, and gave golfers a practical law for their guidance by saying that the ball will always try 'to follow its nose': that is to say, the ball will always deviate in the direction of the spin of its leading point. He then suspended a rotating disc by a rod and swung it as a pendulum to show that it confirms Lord Rayleigh's explanation of the cause of the golf ball's deviation, by deviating in the same manner.

The fact is that Lord Rayleigh's explanation is equally valid, whether applied to that rotating disc or to the golf ball; but Lord Rayleigh has done a lot of work in the Royal Institution in the course of the intervening twenty-three years, and I venture to say that he does not himself continue any longer to believe in the explanation of the golf ball's deviation which he invented in the year 1877.

In the Friday Evening Lecture of the 29th of April in the year 1899, Professor Andrew Gray swung a rotating disc as Sir J. J. Thomson did yesterday; but Dr. Gray's disc was covered with a metal case, so that the air inside the case swung with the disc and prevented the creation of the differences of pressure which are the basis of Lord Rayleigh's explanation.

I think it must be admitted by every scientific man that no explanation of the deviation of Sir J. J. Thomson's disc can reasonably be accepted as valid unless applicable to Dr. Gray's covered disc.

In connection with the diagram on page 116 of my 'Essays on the Action of Astral Gravitation in Terrestrial Phenomena,' I have shown how the action of astral gravitation resisting all motion with a force as the square of its velocity explains the deviation of Dr. Gray's disc as well as that of Sir J. J. Thomson's disc and that of the golf ball; and, on behalf of my client, I challenge Lord Rayleigh to give any other explanation that covers both those discs as well as the golf ball.

I say there is no fulcrum nearer than the stars for the invisible bonds whose tension resists the motion of each particle of the golf ball.

28th March, 1910.

The Journal of the Royal Statistical Society for this month contains Mr. Sauerbeck's 'Tables of the Course of Prices during the year 1909'; and the information which is elaborately detailed in his statement invites, or to say the least, affords a useful opportunity for supplementing the letters which I addressed to you on the 14th and the 20th of August of last year.

Mr. Sauerbeck's Tables show that during the ten years ended with 31st December of last year the average of prices was 34 per cent. lower than during the ten years ended with the 31st December, 1827 (82 years back), and 27 per cent. lower than during the ten years ended with 31st December, 1877 (32 years back).

What does that mean?

One of the facts it tells us is that, roughly speaking, each amount of interest paid by the nation on the National

Debt gave its recipient one quarter larger share of the national wealth in the decade just ended than given by the same nominal amount in that ended with the year 1877.

As I hold it truth with him who says in honour shall a kingdom be established, I say in honour's name let us equitably pay the interest we have contracted to pay; but why, in the name of common sense, have we persisted in legislating in such a manner as to make ourselves virtually pay during the decade just ended an amount equivalent in value to about sixty million sterling more than we contracted to pay to our creditors ? [1]

The worst of it is that the loss which the nation imposes on itself by legislating in such a manner as to enhance the value of the money it has engaged to pay away, is a trivial matter in comparison with the mischief done by the artificial increase of the standard of value. In proportion as the standard is enhanced in value every farmer who has borrowed money to help in the development of his property, and every enterprising man who has borrowed money to help him to establish a factory or to run a cab or an omnibus, finds his estimates of profit more or less thwarted because the rising value of money insidiously gives his creditors a larger share of the profit of his undertaking than was intended by him or expected by the lenders.

Whilst the agricultural and industrial classes (who are the backbone of the country) are handicapped in the above manner by estimated profit not being realised by themselves, that miscarriage of the profit accruing from their enterprise causes a restriction of their demand for labour, and the wage-earning classes must to a greater or lesser extent become unable to find employment.

Mr. W. H. Mallock, in the *Times* of 29th January of last

[1] That question is answered over and over again in 'The Standard of Value.'

year, commented on some statements by Mr. Keir Hardie to the effect that the annual income of the country available for clipping by the tax collector was then £147,000,000 greater than in the year 1900, whilst at the same time there was a decrease of £1,300,000 in the aggregate annual income of the wage-earning classes.

I do not know that the figures commented on by Mr. Mallock have been challenged, but whether quite accurate or not, they illustrate the action of an artificial enhancement of the standard of value in thwarting the otherwise normal demand for labour, whilst at the same time enhancing the capital and the income of the money-lenders. Many who under normal conditions would be able-bodied labourers are rendered by poverty physically unfit, whilst others roll in wealth forced on them beyond their expectations—'Ill fares the land, to hastening ills a prey, where wealth accumulates and men decay.'

A radical error in Mr. Keir Hardie's philosophy is that he supposes the labourer to be victimised by the man who employs him, whereas in fact the employer is the first victim, and it is because he has been victimised that he cannot continue his demand for labour. The actual employers and employed are in the same boat, and must pull together or drown together.

The subject tries my temper too much, and I am sure you will excuse me for refraining from further remarks pending a restoration of equanimity.

29th March, 1910.

I resume the aggravating subject regarding which I have just posted a letter to you, feeling that it more urgently demands the attention of scientific men than that on which my letter of 19th instant challenged the Royal Institution's Professors of Natural Philosophy.

In my letter of 14th August of last year I alluded to the fact that during two or three years following the 28th July, 1880, on which my estimate of 20 pence per ounce was given as a practical limit to the fall in silver which the attempt to establish gold monometallism might cause, the market price, which was then about 51*d.*, rose to about 53*d.*

That rising value of the white metal gave the clever advocates of the gold standard two years in which to smile at the arguments by which a possible fall from the price of 51*d.* to 20*d.* was estimated. But an alternation of ups and downs in the market resulted in the price of $21\frac{1}{16}$ being quoted in the year 1902, and now Mr. Sauerbeck's tables show $23\frac{11}{16}$ to represent the average of prices during the year ended with December last.[1]

It is sad to think that many who considered the course of prices in 1880 to 1882 a practical *reductio ad absurdum* of my arguments are not now living to admit that the aggregate of thirty years is a very practical justification of those arguments.

The reason why it is expedient to record the above facts is because I am now again as in July, 1880, whilst the immediate course of the market price seems to be upwards, endeavouring to show that if the now prevailing course of legislation is persisted in it must result in a further fall of silver, which will be disastrous throughout the commercial world, especially in gold-standard countries, because though the fall now threatening will probably be partly due to an actual fall in the value of silver, it will be chiefly due to a rising value of gold, in which the price of silver is quoted.

Our gold sovereigns were made legal tender in India in the year 1899 ; and, according to Mr. Atkinson's paper

[1] In the original I gave $21\frac{11}{16}$ by mistake for $23\frac{11}{16}$. That does not however, affect the argument, as $23\frac{11}{16}$ is in fact the lowest average on record.

in the *Statistical Journal* for September of last year, less than one million were absorbed by the Indian circulation during the course of the next four years; but then their absorption steadily, year by year, increased until in the year 1908 it amounted to more than 26,000,000 sovereigns, which I understand to be now disappearing from circulation through being hoarded instead of silver rupees.

As the Indian Mint has for some years been closed against the free coinage of silver, the hoarding of sovereigns instead of rupees is a natural consequence of our currency legislation in India.

The fact is that, whilst many clear-headed business men in Europe and in the United States were calling attention to the looming danger of disastrous scarcity of gold, our legislators in India, supported by others in London, were doing all they could to make the Indian people prefer gold to silver, to the detriment not only of the silver standard in India, but also to the detriment of our gold standard: in the one case by artificial depreciation and in the other by artificial appreciation, the object aimed at by at least some of our financial legislators being to make the gold standard prevail throughout the commercial world.

John Locke more than two hundred years ago told the Chancellor of the Exchequer that 'when the quantity of money is lessened the price of things must fall. The labourer feels it next. For when the landlord's rent falls, he must either bate the labourer's wages, or not employ. . . . If one-third of the money were gone out of England, scrambling amongst ourselves helps no more against our want than the pulling off a short coverlet will amongst children that lie together preserve them all from the cold. Some will starve unless the father of the family provide better and enlarge the scanty covering.'

What would John Locke say to our Gold Standard advocates who have been inducing India to pull at our 'scanty covering,' not because that country is in want of 'blanket,' but because we have so legislated as to make them want our gold blanket rather than the ample covering provided by their silver blanket?

The manner in which our legislation in India has led that country, and also some business men in China, to fancy our gold blanket desirable for those countries, and the fact that India is already appreciably pulling it from us, makes it more than ever imperative for us to pull at the silver blanket which is just at present superfluously large for them. Our agricultural and industrial classes have a right to the inclusion of silver in our standard of value, and the influence of our banking community cannot much longer thwart that right without entailing disaster all round.

14th April, 1910.

On re-reading my letters of 30th June and 12th July of last year, I notice a point of historical detail regarding which it is for important reasons desirable that misconception should not be allowed to become prevalent.

The relative positions of Lamarck and Charles Darwin as pioneers in the evolution of scientific knowledge, though it does not affect the object of my argument in those letters, is of itself an important matter in the history of science, and may perhaps be more explicitly stated than has, I think, appeared in the 'Cambridge Essays.'

The great point at issue eighty years ago in the midst of the battles in which St. Hilaire was the champion of Lamarck was: is Fixity of Species, or is Evolution of Species from antecedent forms, true in nature?

Lamarck contended that long persistent desire and effort to develop higher attainments caused higher species to be developed from inferior species : in the words of one of our great poets, he held that life could rise on stepping-stones of its dead self to higher things ; and Charles Darwin brought forward the idea that Natural Selection causes the development of species from antecedent species. They used different arguments, but were both fighting on the same side in the same battle against the idea of the Fixity of Species. The difference between them was a detail of the main question ; and the relative merits of the two different interpretations of matters of fact—or of views which must be brought to issue by matters of fact—does not appear to me to have been yet definitely decided.

That is to say, they were both advocates of the idea of development throughout the organic world from low to higher types of species in opposition to the idea of each species having been independently created. The method of evolution, as to which they differed, is a detail of the main question ; and the scientific world is not yet definitely agreed as to the relative merits of the details which remained at issue between them.

It seems to me that a very interesting historical question which still remains unsettled is as to the relative positions of Erasmus Darwin, Lamarck, and Goethe, as originators of the idea of organic evolution ; or, in other words, as opponents of the idea of the fixity of species. St. Hilaire and Charles Darwin were followers in their footsteps ; and when the great war commenced between Cuvier and St. Hilaire, Charles Darwin, if a partisan at all, must, I think, have been on Cuvier's side, influenced by the same considerations which kept my father and a host of thoughtful men for many years on that side.

15th April, 1910.

I find another point in my 'Sling Letters' calling for explanatory comment beyond that dealt with in my letter of yesterday. On re-reading my letter of 29th March, I have become confirmed in an opinion which has long been developing in my mind to the effect that I have perhaps achieved a record which gives me a pre-eminent position in science as a maker of mistakes. The mistake in that letter does not, however, affect the point immediately under discussion, though if used as a basis on other points it might lead to erroneous conclusions.

I have in that letter given $21\frac{1}{16}$ instead of $23\frac{1}{16}$ as the average of the market prices of silver during the year ended with the 31st December last, but as even the higher of the two figures, $23\frac{1}{16}$, represents the lowest annual average on record, the difference does not affect any part of the argument of that letter, and I am pleased to think that my mistakes have generally, if not always, held a similar position in relation to the arguments with which they have been connected.

The point is that thirty years ago, when silver was quoted at 51*d.*, I estimated 20*d.* as a practical limit to the fall in price which might result from an enforcement of the ideas of the advocates of the gold standard, and the rise to 53*d.* during the next two years encouraged them to be merry with my estimate, though the prospect of a temporary rise is in fact foreshadowed in the same argument as that which gave the estimate of 20*d.*; but we now, after the lapse of thirty years, have before us the fact that last year's prices give the lowest average price on record for any complete year, so that all adverse argument based on the rise from 51 to 53 in two years is refuted by the fall to $23\frac{1}{16}$ in twenty-nine years.

The supply of gold during those years has greatly

exceeded anything anticipated thirty years ago, and has enabled the gold-standard advocates to play the pranks they have been playing with the Indian currency without entailing immediately the disaster for which latent causes are now accumulating, and against which the restoration of our Double Standard is the only honest protection, whilst forming at the same time a clearly defined course of action.

P.S.—I am sure, Sir William Crookes, that you will give me credit for knowing that the sure course for avoidance of mistakes is to refrain from doing anything; so that the merits of any work must be determined by the side on which it shows a balance between truth and error and the value of that balance.

25th April, 1910.

In the able and very interesting lecture delivered last Friday evening by Mr. Thorne Baker on the Telegraphy of Photographs there was the same erroneous use of the term inertia as in Mr. Duddell's lecture on which I commented in my letter of 9th January. The argument of that letter is equally applicable to Mr. Baker's use of the term inertia, and I will not rewrite.

Mr. Baker used the terms *resistance* and *inertia* throughout his lecture as if they represented intrinsically opposing forces, though he knows, I am sure, as you and I do, that according to all schools of philosophy 'resistance,' as the term was used by him, is a consequence of inertia.

It is only by the acceptance of Galileo's idea of matter being indifferent to rest or motion that the term inertia can be applied to the force of motion as well as to the force of resistance; but even under that philosophy it is inexpedient to use a term which is equally applicable to either of the forces to represent one only—the force of motion—when in

fact it is also the force of resistance which is opposing the motion.

Rankine introduced the term Energy for use where Mr. Baker says inertia, though he did not consider Mr. Baker's use of the term intrinsically incorrect; and Kelvin more accurately defined it as Kinetic Energy. The only quarrel I have with the latter term is that vis viva had already for hundreds of years been used for the same purpose and is, to say the least, quite as appropriate, so that the new term was not wanted. The fact is that Galileo's clever but erroneous application of 'inertia' to the force of motion created a confusion which called for new terms which really are not wanted.

Half of Rankine's mighty intellectual power was wasted in struggles to unravel that confusion without ever thinking of cutting at its root. I beg you, Sir William Crookes, to earn for yourself the gratitude of generations of students yet unborn, by leading the Royal Institution to recognise that inertia is merely the opposite aspect of universal gravitation—it is an inherent property of matter by virtue of which the material universe resists all motive forces acting on it, and never itself a force of motion.

24th May, 1910.

Professor Love, of Oxford, in the address delivered in our Institution to-day, gave an airing to Lord Kelvin's argument to the effect that the earth is solid—'not fluid, as has been very generally supposed recently.'

Please kindly allow a humble member of the Institution to point out that, as an answer to the idea of the earth being now in a fluid state, Lord Kelvin's argument is obsolete—it constitutes in fact no answer at all. In a paper on the manufacture of Steel Rails which I presented to the

Institution of Civil Engineers on the 21st of December in the year 1900, I virtually gave an answer to Lord Kelvin's argument in the words :

' The metallic atoms in every steel or iron rail throughout every railway line are in a constant state of motion which, though extremely slow, corresponds with the action of gravitation, under different conditions, but nevertheless identical with that which I described as making it impossible for any fluid particles to be otherwise than in a constant state of circulation. There is in fact no hard and fast line between liquid water and solid steel ; the water is merely less solid than the steel, and the steel less liquid than the water.'

That the great mass of the earth is fluid or elastic—not a rigid solid—is shown by the fact that its axial rotation gives it the same oblate spheroidal shape as the ocean. We—that is to say, the advocates of a fluid earth—are not concerned with refuting Lord Kelvin's argument to the effect that the earth is as rigid as steel ; we merely say, Very well, if that is so, then steel is sufficiently fluid, pliant, or elastic, to bend or flow under the action of the earth's rotation from a normally spherical form to the same oblate spheroidal form which that rotation gives to the ocean.

I, however, think that Lord Kelvin's arguments will have to modify themselves into agreement with Edmund Halley's opinion to the effect that the interior of the earth rotates with greater angular velocity than its outer crust, though we do not yet know whether the property by virtue of which it is enabled to do so ought most properly to be termed fluidity, pliancy, or elasticity ; or even by the separation of the outer crust from one or more interior shells as supposed by Halley.

I have held back the above for the concluding lecture,

and have nothing to add except an expression of my indebtedness to Professor Love for his two most interesting addresses.

28th May, 1910.

The explanation of the Sling Forces given in Part I. and summarised in the Preface to Part II. gives a suitable basis for reconciling an important point in Newton's 'Principia' with the action of astral gravitation in natural phenomena. It is mere matter of fact that in his demonstration of the Law of Gravitation Newton treated velocities as forces. A stone dropped from the hand falls 16·1 feet in a second, and he found that the moon falls 0·00447 of a foot every second from the tangent to its orbit at the beginning of the second. The moon's fall is thus 3600 times less than that of the stone, and as the moon is sixty times farther than the stone from the earth's centre, the fall is therefore inversely as the square of the distance.

Newton accepted that as a direct proof of the Law of Gravitation, and inferred from it that interplanetary space is void of resistance, thus enabling the ratio of motive forces to be also that of the resultant velocities.

The Law of Gravitation is certainly inversely as the square of the distance, and the moon's fall every second from the tangent to its orbit is certainly a valid proof of that law, but the proof is not so direct as supposed by Newton. The moon's real motion is along its orbit—not from the tangent—and all such motion in space is as the square root of the force by which it is caused. That is a necessary consequence of inertia being merely the opposite aspect of universal gravitation resisting all motion with a force as the square of its velocity, so that the resistance of inertia to motion is as universal a law as the Law of Gravitation.

To illustrate the foregoing, take the diagram of the Sling Forces on page 60 of Part I. and, instead of 1570 feet, take first the orbital velocity of Mercury, 221,000 feet per second ; and secondly, that of Mars, 111,700 feet per second. Those velocities are inversely as the square roots of the respective distances from the sun, and, therefore, by theory require the sun to exert a revolving force inversely as the distance ; and in fact in a pamphlet which I published in Buenos Ayres in the year 1870 I gave that as the revolving force of the sun's gravitation, because a Table published in the pamphlet showed that the fraction of the sun's force which acts as a revolving force is inversely as the distance.

For a complication of reasons I was led to modify that in a Second Edition of the pamphlet, and made the revolving force inversely as the *cube* of distance by the consideration that though the fraction of the sun's force which acts as a revolving force is inversely as the distance, the parent force is itself reduced as the square of the distance. That argument is quite true as far as it goes, but it overlooks the fact that the fraction of the sun's force shown in the Table as *opposing* the revolving force is also inversely as the square of the distance, so that the free revolving action of the sun's gravitation is truly inversely as the distance from the sun, and the orbital velocities of the planets are in fact directly as the square roots of the revolving force of the sun's gravitation in their respective orbits.

The so-called fall from the tangent is not a real motion ; the only real motions are along the orbits, and those velocities are as the square roots of the forces of solar gravitation by which they are caused if—and only if—the direct force of gravitation is inversely as the square of the distance as declared by Newton.

I forty years ago abandoned the force inversely as the distance in deference to the necessity of having a transverse

force of solar gravitation inversely as the cube of the distance, not thinking that I could have both ; but in fact the force inversely as the distance is the effective revolving action of the force inversely as the cube of the distance, and the latter is a consequence of the direct force being inversely as the square of the distance.

30th May, 1910.

The address delivered on Friday evening the 4th of February by Professor William Bateson, of Oxford, on ' The Heredity of Sex ' gave a basis for thought beyond the guess-work vaguely suggested by the title of the lecture.

In the year 1866 Messrs. Longmans & Co. had drawn for my work, 'An Investigation of the Forces which determine the Position and the Movements of the Ocean and the Atmosphere,' two sketches in illustration of the action of those forces in the evolution of the human frame. I do not remember who the artist was, but thought them very well drawn and that they would, besides being the only artistic ornament of the book, become regarded as its crowning achievement in science. One of the pictures was a young Hercules with the lines of forces, depicted on the cover of the book and since reproduced in the frontispiece of my ' Essays on the Action of Astral Gravitation in Natural Phenomena,' adjusted to his torso ; and the other a sphinx to indicate that the precise reversals requisite for adjustment to the female figure remained to be solved.

I was then imbued with the idea that, besides the reversed combination of forces in the torso, a reversed system of connection between the brain and the body was necessary ; but whilst listening to the important series of lectures on The Brain delivered by Professor Frederick Mott in our Lecture Hall last year, I decided that to be an erroneous idea, and I then regretted that under its impulse I ruthlessly

destroyed them, with an accumulation of other stock, six years ago, after they had been hanging in abeyance for thirty-eight years.

The lectures by Professor Mott and Professor Bateson above alluded to now give those vanished sketches an importance which justifies or calls for comment on some important considerations which they suggest.

Not many years ago our illustrated papers contained pictures of cricket matches between men and women; and it was supposed by some among them that equality of practice would enable the latter to play on terms of equality against the former; but a consideration of the difference in the combination of forces in the upper and lower parts of the torso must, I think, make it evident that the fact of the game having been developed in such form as to test to the utmost the physical strength, as well as the physical and intellectual skill of full grown men, must make it, for that very reason, unsuitable for the display of the highest forms of physical and intellectual strength and skill in women. They cannot act as rivals or competitors on fair terms in any matter which taxes to the utmost the special characteristics of the physical and intellectual power possessed by either, for nature has given them complementary parts to play, not positions of rivalry, for the guidance of the world.

11th June, 1910.

'The Sling Letters' have now been running for more than five years, and as I do not suppose you will consider that long enough to give you a prescriptive right to their continuance, I anticipate no opposition against my intention to conclude them with the publication of Part V.

The occasion which called for the first letter of the series was the attempt of the Royal Institution officially

to validate the modification of Newton's Law of Gravitation which had for some time been under discussion in the scientific world. The rebellion against that law may now, I think, be safely considered as so thoroughly crushed that there is no longer any risk of science getting into the wrong track along which it would have been impelled by accepting the proposed modification of Newton's law.

Rightly or wrongly, I am confident that 'The Sling Letters' have also done much towards leading to the acceptance of the law to the effect that the inertia of the material universe causes each particle to resist motion with a force as the square of velocity just as certainly as that each particle attracts every other particle with a force inversely as the square of distance.

The letter which I addressed to you on 19th March last in reply to Sir J. J. Thomson's recapitulation of Lord Rayleigh's explanation of some vagaries of golf balls, and that of 11th April, 1906, in reply to Sir J. J. Thomson's explanation of a gyroscopic phenomenon which he showed us in one of his lectures, stand unanswered.

Sir J. J. Thomson's corpuscles will have to give way to the law of inertia for explaining gyroscopic action ; and that law explains in the same manner the vagaries of the golf ball dealt with by Lord Rayleigh.

A straighter challenge was never given than that which I have given to our official Natural Philosophers on the above points on behalf of the Law of Inertia ; and as Sir James Dewar when in Cambridge is a Natural Philosopher officially, though 'only a chemist' in our Institution, he may fairly, I think, be entitled to consider himself included in the challenge.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

Letters to Dr. J. H. T. Tudsbery, Secretary, the Institution of Civil Engineers

22nd January, 1910.

Dear Sir,—I have just read the elaborate and remarkably clear Board of Trade Report by Colonel Von Donop on the accident to the London and North-Western Express at Friezland on 10th August last.

No mere member of the public thoughtfully considering the record of facts stated in that Report could, I think, refrain from a feeling of satisfaction at the evidence they give of able and honest work among those responsible for the working of the line.

Colonel Von Donop's failure to find any fault in the line or in its management has forced him to the conclusion that 'the engine was not of a suitable construction for running round the Friezland curve at the high rate of speed at which it was travelling'; and he suggests that the overhang of nearly 11 feet at the trailing end of the engine would probably start a tendency to oscillation whilst running down the *falling gradient*; but he does not explain how that overhang becomes a danger at high speeds.

If you (that is to say, those members of our Council who are in positions of responsibility for railway management) will look at the letter I addressed to you on 27th January, 1909, and consider the practical experiment described in the diagram given in that letter, you will find a definite exposition of the mechanical action by which the overhang must be always tending to make the leading

outer wheel climb the rail when *rounding a curve*, as it was doing when the accident occurred.

I have since published that diagram in Part III. of 'The Sling' (see p. 184), which is in your library. The overhang increases the tendency of the mass at the trailing end of the engine to carry the trailing wheels towards the inner rail and so to increase the pressure of the leading wheel against the outer rail.

26th January, 1910.

With great diffidence I beg to be allowed to submit to the Institution of Civil Engineers an exposition of an important question which Colonel Von Donop does not appear to have considered to be involved in the Friezland disaster. The fact that part of the weight of the engine was not rigid material, but water, has not been considered by him as a matter which could have any important action in connection with the disaster.

What is the difference in the force acting on an engine entering a curve with the water in its tank frozen all solid and the same engine in ordinary conditions?

It is that in the latter case the centrifugal force (*pace* Thomson and Tait and all their present disciples) is augmented by the momentum of the heave of the water towards the outer rail, creating an extra pressure outwards against the side of the tank proportional to the square of the velocity with which the water heaves across the tank.

If that pressure were steady and persistent all round the curve and not sufficiently strong to capsize the engine it would simply tend to keep the wheels firmly pressed against the outer rail and be an element of safety rather than of danger.

Suppose, however, that the vis viva of the thrust given

by the water is only just enough to carry the rear outer wheel against the elevated outer rail; then, as the water becomes steady in its new position, the vis viva of its motion ceases and the rear wheels will tend to fall from the super-elevated outer rail to the inner rail, and the jerk of the falling motion of the inner rear wheel against the lower rail might well lead to a sudden lifting of the outer leading wheel over the outer rail. That might be the easiest rebound of the falling motion against the inner rail.

In the Friezland accident the outer leading wheel did in fact mount and cross the outer rail very soon after entering the curve; and in the absence of any other apparent cause for the derailment I venture to suggest the action above described as worthy of consideration. The inference to be drawn from the argument, *quantum valeat*, is to the effect that any excess of superelevation of the outer rail beyond that essential at any given speed is less likely to become an element of danger with ordinary than with tank engines.

23rd February, 1910.

Anyone who has ever been Managing Director of a railway construction company, as I have been, must take a more or less keen interest in accidents in an instinctive desire to discover contributory causes for the disasters.

In each of the two letters which I addressed to you on 22nd and 26th ultimo respectively, I suggested a contributory cause for the accident to a London and North-Western Express at Friezland on 10th August last. A reconsideration of those letters which I had reason to make yesterday has made me confident that in each case I gave a suggestion new (as far as I have been able to ascertain) to railway managers and builders, and which must of necessity sooner

or later be acknowledged by all as correct, and then be availed of as a guiding principle.

As that accident occurred on a curve, it recalls to mind the three weeks' discussion of Railway Curves in our Institution in January of last year. I think my memory serves me correctly when I say that I have never met, either throughout that discussion or elsewhere, with any consideration of the relative merits, when constructing a curve, of raising the outer rail from its normal level, or lowering the inner rail to give superelevation to the outer rail. I have, however, in a paper entitled 'The Scrapping of the First Law of Motion' which I presented to you in January, 1907, pointed out incidentally that by lowering the inner rail to suit any given velocity gravity might be made to do all the extra work requisite for rounding a curve without reduction of velocity or increase of engine power.

When the outer rail is elevated it has not only to do the work of turning the train by resisting the horizontal pressure of the wheel flanges against it, but it has also to do work vertically to lift the train from its previously normal line of motion. Each of those actions creates an important stress on the rail which can be avoided by lowering the inner, instead of elevating the outer rail, so allowing the work to be done by gravity instead of by rail resistance to the motion of the train.

The difficulty is that a train, taking at a speed of twenty miles an hour a curve adjusted to sixty miles by lowering the inner rail, would be more liable to be brought to a stand in the curve by friction against the inner rail, or even perhaps to be capsized inwards from the curve.

I will not attempt a general consideration of the question, but limit myself to suggesting that as the Friezland accident occurred on a falling gradient it would be easy for any train to enter that curve with sufficient speed to round it if

adjusted to the speed of express trains by a lowering of the inner rail.

1st March, 1910.

I have to acknowledge receipt of your letters of 18th and 25th ultimo, which I have read with much interest.

Commencing with the paper on Steel Rails dated 21st December, 1900, the campaign of ten years through which I have endeavoured to make the action of astral gravitation officially recognised in our Institution gives a splendid retrospect from my point of view, with brilliant prospects foreshadowed in the coming decade.

What I regret is that I cannot see how any member of our Council can regard it complacently.

Quousque tandem——

17th March, 1910.

I have recently noticed an important illustration of the main argument of my contribution to the discussion of Professor Vernon-Harcourt's paper on the Hooghly River, read on the 17th January, 1905; and as it illustrates on a greater scale and in a manner which cannot, I think, fail to be of interest to all Hydraulic Engineers the action which I described as creating sand-banks in the Hooghly River, I venture to submit it for consideration.

The current which enters the Mediterranean Sea is an offset from the North Atlantic anticyclone, and tends to create a cyclonic circulation in the Mediterranean. So also the current which enters the Red Sea is an offset from the anticyclone of the Arabian Sea, and tends to take a cyclonic course of circulation in the Red Sea. If the barrier between the Mediterranean Sea and the Red Sea were removed, so as to allow those two independent cyclonic streams to meet freely, they would immediately begin to

redeposit sediment on the west side of the opening between the two seas, exactly as I described in the Hooghly River; because each stream would be incessantly endeavouring to preserve its cyclonic character, so that the bank, first formed at their junction, would gradually be extended eastwards and ultimately reclose the connection between the two seas.

The mistake made by your 'Publication Committee' in excluding my views from their published record of the above-mentioned discussion is deplorable, but they are now recorded in Part II. of 'The Sling,' which is in your library.

19th March, 1910.

Twice in the course of a series of lectures on Electric Waves which Sir J. J. Thomson has just concluded in the Royal Institution I have been forcibly reminded of the paper on Steel Rails which I submitted to you on the 21st of December in the year 1900.

The connecting link between the paper on Steel Rails and Sir J. J. Thomson's lectures was not forced on me by any scientific opinion expressed by the lecturer, but merely by well-merited allusions to the pre-eminence of the late Professor Heinrich Hertz, of Berlin, as a pioneer in the abstruse questions dealt with in the lectures. The main reason for the pre-eminence of Dr. Hertz in his scientific investigations I consider to be explicitly given in my above-mentioned paper, where I pointed out that he had practically, though not avowedly, abandoned Newton's so-called First Law of Motion.

Dr. Hertz's abandonment of the theory of inertia on which that law is based brought him out of a wrong road along which the great majority of his fellow-workers were pursuing their investigations, and enabled him better to

appreciate the relative value of facts, or to grasp their indications as they came before him; and the manner in which the Royal Geographical Society, the Royal Society, the Royal Institution, and the Institution of Civil Engineers have persisted in preventing anything avowedly at variance with Galileo's idea of inertia from appearing in their official records has become a national disaster.

6th April, 1910.

Please kindly allow me to express my admiration of the thorough manner in which the unnecessary and mischievous vibration stress pointed out in my letters of 15th April and 12th May, 1908, in connection with the King Edward VII. bridge over the Tyne, has been avoided in the construction of the new Clyde bridge at Glasgow, which was described in the paper read last evening.

14th April, 1910.

In my letter of 21st December, 1900 (nearly ten years ago), commenting on Sir Lowthian Bell's paper on the Manufacture of Steel Rails, I pointed out what appeared to me to be an important mistake, and in my letter of 24th February, 1906, I further pointed out that experiments which had just then been made by Professor Arnold confirmed the views I had expressed. Those experiments in fact made the views which I had challenged obsolete, and every member acquainted with the facts of the case and jealous for the reputation of our Institution must be more or less saddened by the thought that the views which have become the basis of the theory by which manufacturers of steel rails are now guided were ten years ago refused a place in our published proceedings.

My reason for alluding to the above at this moment is to prevent my letter of 6th instant on the Vibration Stresses

in the King Edward VII. bridge from appearing to stand alone, instead of being merely one among many points on which I have found it expedient to challenge the judgment of our Publication Committee.

As a pendant to my letter of 6th instant I now record that in the course of the discussion on Masonry Dams on 4th April, 1905, we were told that the builders of the Assuan Dam had become puzzled how to deal with the erosion of the river bed below the dam, and I suggested in a letter to Sir Benjamin Baker the following morning that if, instead of allowing the surplus water to escape smoothly from the valves, the jets were directed against each other, they would mutually destroy the momentum given to each jet by the head of water in the dam, and so do away with the rush of water which caused the erosion of the river bed.

I gather from Sir Benjamin's reply that he did not think the suggestion practicable, but I understand that some such arrangement has since been adopted, though he is no longer with us to recognise its value.

Presuming that I am right in understanding my above suggestion to have been adopted, and to have proved a successful remedy for the trouble which vexed the engineering staff of the great dam five years ago, I ask that it may be borne in mind that I allude to it merely as a relatively minor point among others on which I have during the past ten years given correct guidance to Hydraulic Engineers, as shown in the four Parts of 'The Sling Letters' already published and honoured with a place in our library.

23rd April, 1910.

I address you to-day for the purpose of giving a broader basis for the pleasure with which you intend to submit my letter of 14th instant to your Publication Committee, as I well know that the idea of my advocacy of the action

of astral gravitation in terrestrial phenomena being an unpardonable offence will have to be abandoned by the Council sooner or later. The sooner the better for all of us.

In my letter of 24th June, 1907, I gave an extract from one of Dr. Elgar's addresses to the Institute of Naval Architects advocating deep draught of water as a most important element of speed at sea, and asserting that if the draught which could be given to fast passenger steamers were not restricted by the depth of water in ports and docks, the resistance to their motion could be reduced by giving them such a form as would increase the draught of water.

At the request of the Council I had on 9th January of the year above mentioned given you a paper which cuts the ground from under Dr. Elgar's above-stated views by showing that the theoretical idea of inertia by which he had been guided, together with William Froude and Sir George Stokes and all their colleagues in Naval Construction, is demonstrably at variance with clear matters of fact; but the Publication Committee, nevertheless, in spite of having my arguments before them, not only in that paper but also in many letters, enabled Dr. Elgar on the 24th June of that year, 1907, to thwart any official consideration of the new views by a parade of re-stating the views which had then already practically become obsolete.

To me it soon became clear that Dr. Elgar found himself in an untenable position, supported by few if any former partisans of his views; and if the Council had then allowed the new views a hearing, instead of bolstering obsolete ideas, he would probably still have been with us, adapting his great technical knowledge frankly to the new theory of inertia, to the benefit of this country.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

Letters to His Majesty's Most Honourable Lords Commissioners, The Admiralty

19th April, 1910.

My Lords,—The *Times* this morning records the stranding of the steamship 'Minnehaha' on one of the Scilly Islands, in foggy weather which had prevented the officers of the ship from taking observations for three days.

That is one more disaster to add to those recorded in my letters of 21st and 31st May, 1907, and in some older letters which I have not at hand to refer to, all due to the same cause, of which their captains had no knowledge: namely, the cyclonic action which gives the Atlantic tide a surface roll northwards as it enters the Bay of Biscay and the Channel.

14*th May*.—This is not a long letter, though it is now twenty-five days since its construction was commenced, as I have not in the meantime been disposed to recapitulate the arguments I have from time to time during the past forty years pressed on your office.

I now remind your Lordships of the existence of those arguments as the manner in which the solution of the problem of oceanic circulation has been ignored by your office is becoming a national disaster. It ought to have formed part of the 'Challenger' Report thirty years ago—*pro patria*—but unfortunately not one of the leading men connected with the Expedition could face the idea of the ocean being circulated by the action of the stars.

6th May, 1910.

In a letter addressed to the Royal Geographical Society on 19th September, 1904, I urged that we had then before us what would probably prove to be the last chance our navy would have of being the first to achieve the task of reaching the North Pole, on which they were the first to venture.

Where are we now? Our efforts to reach the Pole have given the venture a prestige which has attracted many bold and able aspirants. The knowledge gained by the achievements of Peary and by the failure of Cook combine to make it appear that every spring season there is for some time an open lane of water stretching from near the north-west of Greenland to near the south-east of Crocker Land, which seems to form a dividing line between a drift southwards and westwards past Crocker Land, and another drift which supplies the ice-floe of the east Greenland coast. There is now a clear field for you, my Lords, to allow our navy a turn in Arctic ventures by way of Smith Sound to explore Crocker Land and to delineate its position, which may quite possibly give sufficient extent to place its northern extremity not far from the Pole. It seems strange that Canada is not now exploring in that direction with the Parry Islands [1] for a base of operations.

Peary's narrative given in the Albert Hall last evening has cleared some difficulty as to previously published accounts and justified the reception given to him by the Royal Geographical Society. What is now described as the work done by him, though it constitutes one of the most splendid records of human endurance, courage, and skill, no longer justifies incredulity; and in the promptitude with which the Council of the Royal Geographical Society awarded their Gold Medal the country will doubtless

[1] Or North Devon and Ellesmere Land.

recognise that they showed sound judgment in what was then a difficult position.

The Polar Problem is, however, not yet solved. It must be remembered that Captain Scoresby, at one time our leading Arctic Explorer, and with him Captain Maury, of the United States Navy, insisted that the ice annually discharged from the polar regions showed that a large oceanic area must exist there, and that its discharge must every summer leave a considerable area of open water in the polar regions.

After many years of exploration their views were justified as regards the magnitude of oceanic area which they demanded, but Peary's record is now treated as disproving their idea of an open, unfrozen summer sea in the far north.

It may be that the open water near Bering Strait and on the coast of Siberia is every summer season enough to satisfy their arguments, but until Crocker Land has been explored and the oceanic conditions at its northern extremity ascertained, the position of the open sea demanded by Scoresby and Maury must be considered an unsolved problem ; and I beg you, my Lords, to reflect as to whether the repute of our country is not now demanding permission for the young men of our navy to be now immediately allowed to venture on a continuance of the Arctic explorations which formed a training school for Nelson.

I have the honour to be, my Lords,

Your most obedient, humble servant,

Wm. Leighton Jordan.

PART VI

LETTERS TO THE ROYAL INSTITUTION
THE ADMIRALTY
AND
THE INSTITUTION OF CIVIL ENGINEERS

JANUARY 1911 TO MAY 1912

IN AFFECTIONATE REMEMBRANCE

OF

THEODORE LEIGHTON PENNELL, M.D., F.R.C.S.,

OF

BANNU, NORTH-WEST FRONTIER PROVINCE, INDIA,

AND HIS MOTHER,

MRS. JOHN WILSON CROKER PENNELL

(ELIZABETH FANNY JORDAN),

AND THEIR KIND INTEREST IN

PRECEDING PARTS OF THIS WORK

LETTERS

To Sir William Crookes, O.M., F.R.S., Secretary, the Royal Institution

ROYAL SOCIETIES CLUB,
St. James's Street, S.W.,
7th January, 1911.

'THE INACTIVITY OF ACTION'

Dear Sir,—*Infandum jubes renovare dolorem.* In the first of the course of Juvenile Lectures which he is now delivering in our Lecture Hall, Professor Sylvanus Thompson drew a fiddlestick across a tuning-fork, then damped the sound and bent and released a steel rod fixed near it so as to put the rod in a state of vibration; and he then told the boys and girls, watching and listening with keen attention, that (1) the inertia of the rod was keeping it in motion; the inference clearly being that the inertia of the tuning-fork was the direct cause of the continuity of the vibration which caused the sound to which we had listened.

And in the third lecture, referring to water used in one of his illustrations, he said 'its inertia (2) tried to keep it still; but when set in motion (3) is keeping it in motion.'

The second of the three statements which I have above numbered is correct, but the others are erroneous. I have in the preceding five Parts of my Sling Letters addressed to you, as well as in previous publications, so fully dealt with the erroneous character of those statements that I am not going now to repeat my arguments, but beg to be allowed to

submit to those who officially represent our Institution in Natural Philosophy a quotation from the Life of Chatham which has just recently been published by Lord Rosebery.

In a speech delivered by William Pitt, afterwards Earl of Chatham, in the House of Commons in the year 1756, he said that ' philosophers had a phrase, *vis inertiæ*, by which they denoted the inactivity of action ' ; and Lord Rosebery's comment on the statement shows him to suppose some misconception on the part of Pitt ; but if you should have an opportunity and be so disposed, you may safely assure Lord Rosebery that Pitt certainly meant exactly what he said. I never before heard of the arguments regarding inertia which became prevalent soon after Newton, and are still prevalent, being so described ; and I incline to think that the statement is probably the result of an original flash of genius after Pitt had pondered over them, though he may perhaps have taken the words from one of his contemporary writers.

I will comment no more on the ideas alluded to by Chatham, but leave you to contemplate his terse and true definition—' The inactivity of action '—as their *reductio ad absurdum*.

11th March, 1911.

Among all the attentive listeners at Mr. Parsons' lecture last evening, I was *perhaps* the only one who noticed the very interesting little point on which I now venture to comment.

When making use of the words, ' the action of inertia in a straight line,' there was an instant of hesitation before the word *inertia*, and when at length the lecturer resolved to make the air vibrate in such a manner as to convey that word to our ears it came in a tone jarringly at variance

with that of the easily confident tone which was otherwise unbroken throughout the lecture.

The action which the lecturer described as that of 'inertia' is really the action of 'momentum,' which is always opposed by the inertia of the moving body. I really feel ashamed that in forty-five years of teaching I have not succeeded in keeping such a mistake out of the Lecture Hall of the Royal Institution.

25th March, 1911.

In a lecture which I delivered in Willis's Rooms on the 3rd November, 1877, I alluded to investigations which Professor Rainey had then recently been making at St. Thomas's Hospital in connection with what are now known in science as Brownian Movements, and I ventured to anticipate that those investigations would at some future time be looked back upon as the first step towards showing what the forces are which cause the circulation of the blood.

The Royal Institution is, I am sure, greatly indebted to you, Sir William Crookes, for having induced Professor Jean Perrin to come from Paris to deliver the very interesting lecture with which we were favoured a few weeks ago on the subject which was attracting the enthusiastic attention of Professor Rainey in St. Thomas's Hospital thirty-four years ago. I did not know, and I do not suppose Professor Rainey knew, that the phenomena he was investigating had already been discovered by Robert Brown in the year 1827.

Professor Perrin says that the work of M. Gouy in the year 1888 showed that the hypothesis of molecular agitation gave an admissible explanation of the Brownian Movements, and that no other cause could be imagined. He further says that it is only from that time that the Brownian Movements

took a place among the important problems of general physics.

I think it expedient for me to comment on some points connected with Professor Perrin's lecture.

In the first place :—It certainly is not my fault, nor the fault of Professor Rainey, that the importance of the Brownian Movements was not clearly recognised by the reputed leaders of science twelve years before the year 1889, which has been specified as that in which its importance became recognised ; and the fact is that Professor Perrin, on the 24th February this year, 1911, in the Royal Institution, attributed to the phenomena the importance which I had given on the 3rd November, 1877, in Willis's Rooms. Professor Perrin evidently does not know that the work which he alludes to as done by Mr. Gouy in 1888 had already been done by Professor Rainey before 1877.

And secondly :—It was because in desultory conversations in the middle seventies of last century I freely stated that the same conflicting action of gravitation which circulates the ocean must also be ceaselessly acting in every drop of water, tending to disturb and circulate its constituent atoms, that I was informed of Professor Rainey's investigations and obtained the favour of becoming acquainted with them. I in the first place recognised a clearly-defined theoretical action tending to cause a constant circulation of the constituent atoms of any fluid, and afterwards found that Professor Rainey's experiments had shown the existence of the phenomena which had been called for by theory. The question now at issue is as to whether Professor Perrin is right in supposing the Brownian Movements to be due to spontaneous action of the particles seen to be moving in the liquid, or to a conflicting action of gravitation which must be ceaselessly tending to create circulation even if each particle be as dead as a door-nail.

4th April, 1911.

Among the experiments which Sir Joseph Thomson has shown us in the course of the lectures which he is now delivering is that which Sir James Dewar described last session as the ' killing of Crookes' Radiometer.'

The facts are simple enough and well known among members of our Institution, but it is nevertheless necessary to begin by re-stating them to enable any reader to appreciate some new views which they seem to support, or with which, to say the least, they are not incompatible.

By the application of liquid air to the radiometer the rotation of the flywheel is stopped, and on the removal of the air the flywheel again rotates just as before the interference occurred. It also seems that there can be no difference of opinion as to the light shining on the radiometer being the cause of the rotation of the flywheel, nor as to the proximity of the liquid air being the cause of the stoppage of that rotation ; but the explanations submitted to us of the manner in which those causes produce the effects are not such as to make it impossible for a mere spectator to have misgivings whilst unable to test them by modifications of their conditions.

I understand Professor Thomson to consider that the action of the light shining on the radiometer causes infinitesimal corpuscles contained within it to bombard the blackened sides of the flywheel and so cause it to rotate ; but that the action of the liquid air increases the vacuum to such an extent as not to leave a sufficient number of corpuscles to make their bombarding action effective.

If not exactly a two-edged sword, that argument certainly strikes in two directions. In the first place it tends to confirm the idea entertained before ever the radiometer was ' killed,' to the effect that the excitement of active corpuscles moving with immense velocity in the radiometer is requisite

to account for the rotation of the flywheel. And, secondly, it tends at the same time to make it appear that the direct action of light in the absence of the imagined corpuscles is not able to set the flywheel in motion.

The question at issue is of such vast importance in its bearing on conflicting theories of light that I venture to point out that the validity of the above argument depends on the manner in which the action of the liquid air increases the vacuum. Does it increase the vacuum surrounding the flywheel without changing other conditions inside the radiometer? Or, if there is such increased vacuum, is it due to humidity being drawn to the inside surface of the glass and becoming frozen so as rigidly to connect the flywheel to its pivot?

I am doubly prejudiced against Sir Joseph Thomson's argument: first, because it seems to me that the rotation of the flywheel must be caused by the direct action of light; and secondly, because the supposed bombarding action by infinitesimal corpuscles calls for too many imaginations. The evidence as it stands to-day, as far as submitted to us at the Royal Institution, is not incompatible with the idea of light being an independent force which can make its way through a vacuum more easily than through any substance, however attenuated.

29th April, 1911.

As the Sling Letters originated on 15th May, 1905, in a defence of Newton's Law of Gravitation against an attempt made in our Lecture Hall by one of our American cousins to modify it, it seems appropriate for me to address to you some comments on 'A Kinetic Theory of Gravitation' which has recently been published by another American cousin, Mr. Charles F. Brush, and reproduced *in extenso* in 'Nature' on 23rd ultimo.

I must in the first place acknowledge the pleasure with which I notice that Mr. Brush accepts Newton's Law of Gravitation in all its simplicity, or, at any rate, he makes no attempt to modify it in any manner, but merely aims at explaining its cause, which he supposes to be æther pressure, or the thrust of wave motion exerted by the æther.

Secondly: As Mr. Brush says he has no doubt used that key clumsily and imperfectly, I must repeat what I urged many years ago to the effect that explanations must of necessity be 'clumsy and imperfect' when anyone, instead of dealing with the direct action of astral gravitation, endeavours to ignore that direct action and deals only with its opposite aspect under the name of the æther, or any other name.

The pressure between a book and the table on which it rests is a consequence of the reciprocal action of gravitation between the book and the earth. Gravitation is the cause of that pressure, and no clear argument logically conducted can possibly make the pressure between the book and the table or any pressure downwards upon the book, whether by wave motion or otherwise, the cause of gravitation. Cause and effect are realities in nature, and cannot be inverted in argument without entailing inextricable confusion.

The 'grip' of the æther on the atom as dealt with by Mr. Brush makes the æther merely the action of universal gravitation—the action of any given atom being one part of a conflicting action of gravitation, and the reciprocal action of all other atoms (their 'grip' upon it) the other part so far as that atom is concerned.

Professor Flinders Petrie's allusion to 'the mystery of the æther' in his lecture[1] last night is the spark which has fired this letter at you, though it would, I think, have been

[1] On 'The Revolution of Civilisation.'

written in any case before long, perhaps at greater length. It is not my fault, Sir William Crookes, that Professor Flinders Petrie does not get clear of that ' mystery.'

Thirdly, I must frankly say it seems to me that Mr. Brush might as well kick against Newton's Law of Gravitation as against the distinction between Kinetic and Potential Energy as understood by Rankine, by Clerk-Maxwell, and by Kelvin. When Rankine defined Actual (Kelvin's *kinetic*) Energy and Potential Energy he might truly have said, as Newton said of his Law of Gravitation, ' *Hypotheses non fingo.*' Rankine will, however, I think, be sufficiently defended on that point without my help.

5th May, 1911.

Dr. Newcomb, in his Article on Gravitation in the new edition of the ' Encyclopædia Britannica ' just published by the Cambridge University Press, says, where commenting on Newton's law, that ' so far as yet known it is absolutely unmodified by any condition or cause whatever.'

Coming not only from Cambridge University but also from so renowned a pen, that statement makes me feel entitled to regard the campaign I inaugurated in my letter to you of the 15th May, 1905, as successfully ended. That law has now, I think, no serious opponent, and Dr. Newcomb's statement ends what I believe to be the last battle it will ever have to fight to maintain its position in natural philosophy.

The campaign in which the opponents of Newton's law have been defeated is, however, merely a subsidiary episode in the vaster campaign inaugurated by the work I published in the year 1866. The object of that work was not to destroy Galileo's definition of inertia, but to show the existence of the action of astral gravitation in terrestrial phenomena,

which could not be done without at the same time sweeping away that idea of inertia, which stood adopted as a basis for the First Law of Motion in Newton's 'Principia.'

The attack on Newton's Law of Gravitation demanded prompt attention as being an attack on the base of the great campaign. It was a surprise attack on the base of operations; and the position of the main question after the defeat of the attempt to disturb its base may now be considered.

In a letter which I addressed to you on 6th May, 1908, and have since published among the Sling Letters, I commented on the position held by Newton's First Law of Motion in the Ninth and Tenth Editions of the 'Encyclopædia Britannica,' which give a full statement of his laws of motion and a dissertation on each of them in the article 'Mechanics.'

I therefore turned with no little interest to that article in the new edition, and find that the laws have been omitted from it, and we are referred to a special article on 'The Laws of Motion' for their discussion.

There also Newton's laws are not given, but we are told that 'such statements as that a body moves in a straight line, and that it has a certain velocity, have no meaning unless the base, relative to which the motion is to be reckoned, is defined.'

As in the article 'Mechanics' we are told that 'the whole subject rests ultimately on the Newtonian Laws of Motion and on some natural extensions of them,' it seems strange that those laws have ceased to appear in the Encyclopædia.

We are told that 'Newton assumed the possibility of choosing' a requisite base for the 'straight lines' of motion. The fact, however, is that Newton supposed the stars to afford a base, *à prendre ou à laisser*—they were Hobson's choice for him; he took the apparent base before him and supposed it to be suitable for his purpose; but the

discovery of the fact that the stars are moving has destroyed that ' Newtonian base,' and has left his first law of motion no such basis in natural philosophy as he imagined to exist.

8th May, 1911.

I have in the course of the last few years seen some remarkably strange statements on the subject of ' absolute motion,' which in more than one case seemed to me to have been deliberately intended to clash with my exposition of the Sling forces as given in Part I. and summarised in the Preface to Part II. of ' The Sling.' They have been casual statements of opinion, but the Cambridge edition of the ' Encyclopædia Britannica ' has, in the article on the Laws of Motion, treated the subject in a more formal manner.

We are told that the establishment of a true Newtonian base presumes knowledge of the motion of all bodies ; and that Newton used the term ' absolute motion ' for motion relative to such a base.

The above idea of Newton's method of dealing with the subject accords with the preliminary part of his argument *if separated from the context* ; but, in fact, farther on, in the course of the same argument, he says :—' It may be that there is no body really at rest, to which the places and motions of others may be referred. But we may distinguish rest and motion, absolute and relative, one from the other by their properties, causes and effects.' . . . ' The causes by which true and relative motions are distinguished one from the other are the forces impressed upon bodies to generate motion.' . . . And ' in a true and absolute circular motion, they are greater or less according to the quantity of motion.'

According to the above extracts the relative motion of the pen and the paper I am using is entirely due to an *absolute*

motion of the pen, in which the paper does not partake; and I think those extracts fairly show Newton's practical common-sense use of the term as applicable to the motion of the pen.

Where I say in Part I. (see footnote, p. 53) 'the absolute velocity of the moon along its orbit round the common centre of gravity of the earth and moon is 3,704 feet per second,' I use the term absolute velocity exactly in the sense defined by Newton.

15th May, 1911.

We all agree that a correct understanding as to what constitutes absolute motion is an important point in the fundamental principles of natural philosophy.

The importance attached to it by Newton is shown by the lengthy argument which precedes his statement of the Laws of Motion in the 'Principia'; and as the Cambridge University Press, in their article on those laws in the 'Encyclopædia Britannica,' have erroneously attributed to him a definition which seems to me to be perniciously at variance with common sense, I find it expedient to amplify my letter of 8th instant in order to make the position of the question at issue quite clear.

If a cannon ball projected horizontally strikes a target at a distance of 4,800 feet in four seconds, it falls 257·6 feet from the horizontal line under the action of gravity. The force of projection gives a horizontal mean velocity of 1,200 feet per second, whilst gravity gives a vertical fall at a mean velocity of 64·4 feet per second; and the mean absolute velocity of the ball along its *resultant* path is therefore $\sqrt[2]{1200^2 + 64{\cdot}4^2} = 1{,}202$ feet per second about.

The potential force of gravity available for the above fall of 257·6 feet from the muzzle of the gun is equivalent to 64·4 feet of work per second for four seconds; and the similar

potential for a fall from the moon's orbit to the common centre of gravity of the earth and moon is 2,358 feet per second for $6\frac{1}{4}$ days. This latter force acts in combination with a transverse force equivalent to 2,856 feet per second exactly as the gravitation force acts on the cannon-ball, and the resultant velocity of absolute motion along the moon's orbit is therefore $\sqrt[2]{2358^2 + 2856^2} = 3{,}704$ feet per second.

I am not concerned now to argue as to whether the above is, or is not, a true exposition of forces acting in the moon's orbit. My point for the moment is merely that, if those forces are truly described, then that velocity of 3,704 feet per second is absolute velocity according to Newton's definition of the term as given in my last letter. He did not give the absolute velocities of any of the heavenly bodies according to his own definition, because he supposed radial and tangental forces to be equal, and therefore obtained from them resultants which manifestly could not be real or absolute velocities. His treatment of the subject under the above formula would have been $\sqrt[2]{2358^2 + 2358^2} = 3{,}334$ feet per second ; and that figure as an absolute velocity is incompatible with the radial velocity of 2,358 feet per second. If that radial velocity is correct it involves the existence of such a transverse velocity of 2,856 feet per second to prevent the moon from falling inwards from its orbit ; and if it is incorrect, then the law of gravitation is not applicable along the moon's orbit in the same manner as along the path of any cannon-ball. *Utrum horum mavis accipe.*

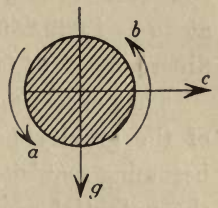
20th May, 1911.

There is some scientific work now being done in the United States which illustrates the importance of the ques-

tion at issue in the theory of the golf ball's motion on which I have challenged our official representatives of natural philosophy.

Professor Kirk, in 'Popular Astronomy' for April, asks: 'What causes the gyroscope to revolve around the pedestal always in the same direction as the movement at the bottom of the disc and opposite to the movement at the top?' And says that the late Professor S. P. Langley employed an expert to determine the mathematics of the phenomenon, who found that the revolution is in fact in the direction opposite to that required by his analysis.

Professor Kirk therefore claims that the gyroscope experimentally demonstrates that gravity is not an attraction from below but a pressure from above towards the centre of the earth, as 'if it were an attraction the disc would revolve in the opposite direction.' And he seems to be now bent on solving the question by differences of the retarding action of gravity in different parts of the disc, evidently not knowing what was long ago explained in my Essay on the Spinning Top.



Let g represent gravity tending to give 16·1 feet of downward motion in one second, a and b the direction of rotation at 1,000 feet per second; then c is the direction in which the gyroscope revolves.

I have, in the above-mentioned Essay, shown that all impressed motion is resisted by gravity, which does its own work in its own way and resists any deviation from that. The particles on the side a, moving downwards at 1,000 feet per second, are going 984 feet per second too fast for gravity, and that excess is therefore resisted as the square of velocity. Against that we have on the opposite side of the disc 1,016 feet resisted by gravity, making it clear that

gravity as a force of attraction between the disc and the earth cannot keep the disc from falling ; but if we turn that action upside down and ignore all other considerations, then the explanation Professor Kirk is aiming at stands already given in my Essay. But can other considerations be ignored ? We are not isolated in the universe, but a material part of it. What is gravity but a local and relatively very trivial part of the force of universal gravitation which connects the gyroscope with every star ? It is that force, for which I can see no better term than astral gravitation, which resists the combined motions caused by gravity and the rotation of the disc on the side *a*, together 1,016 feet per second, and on the side *b* has only 984 feet to resist. The resistance therefore is $1{,}016^2$ at $a = 1{,}032{,}256$, and 984^2 at $b = 968{,}256$, making a difference of 64,000.

That excess makes it necessary for only a small fraction of the gravity potential of 16·1 feet for the first second becoming requisite to support the disc. A small fraction of the potential energy converted to kinetic creates sufficient resistance on the side *a* to support the gyroscope and revolve it in the direction of the side *b*, on which there is less supporting force. By the motion of revolution the more strongly supported side is incessantly being pulled over that which is more free to fall.

The reason why the gyroscope revolves against the mathematician's demonstration is because it is in fact controlled by the reaction of astral gravitation which resists its motion, and not by the direct action of the motion. Professor Kirk, however, gets away from that by inverting the action of gravity, and so makes the reaction of gravity against the rotation of the disc tend to move it upwards instead of downwards. By turning gravity upside down and ignoring the vast forces of gravitation by which we are

surrounded, he makes gravity exert precisely the action I have ascribed to astral gravitation. The reaction with which he deals is simply the resistance of gravitation to the rotation of the disc, as explained in my lecture of 3rd November, 1877, in Willis's Rooms, and which I had given in the year 1866 as circulating the ocean by resisting the earth's rotation.

The gyroscopic pendulum exhibited by Professor Andrew Gray in our Lecture Hall on the 29th April, 1899, confirms the explanations above alluded to as given in 1866 and 1877, and at the same time gives a simple and practical refutation to the explanation given by Lord Rayleigh and Sir J. J. Thomson of the vagaries of the golf ball.[1]

30th May, 1911.

Every boy knows that his top as it spins is kept from falling by the motion of rotation he has cleverly given to it. The point in connection with that which concerned me on 3rd November, 1877, at Willis's Rooms, was merely to show that the spin of the top supports it, not by the direct action of the motion, but by the action of resistance to the motion which is exerted by the force of astral gravitation which surrounds us.

After the lapse of thirty-three years Professor Kirk (as stated in my letter of the 20th instant) has also decided that the top is supported by reaction against its motion, and not by the direct action of the motion, thus agreeing with what is really the most important point involved in the question ; and he has been driven to that conclusion whilst engaged in a special attempt to solve gyroscopic action, whereas I attended to the spinning top in 1877 merely for the purpose of showing that the same force of astral

[1] That is to say, it is not a complete explanation.

gravitation which I had dealt with as resisting the earth's rotation supports it against gravity.

As astral gravitation drags westwards against the earth's rotation, in the same manner as it drags against the top's rotation, a westward pressure is created in the ocean and circulates the water over the earth's surface, because the greater pressure in the equatorial regions where the rotation is fastest overwhelms that in higher latitudes and reverses the normal course there, so as to carry a volume of water eastwards in those latitudes equal to that which is dragged westwards in the equatorial regions.

Those main features of the circulation I gave in charts and sections published in 1866, and with more detail in 1885. Since that I have found practical records corroborating all the theoretical currents of the last published chart which had not previously been practically confirmed.

In face of that I have now to read, in Sir Archibald Geikie's address as President of the Royal Society at their last Anniversary Meeting, that they continue ' to welcome every serious addition to natural knowledge, from whatever branch of science it might come.'

If that is true, then my scientific work from the year 1854 to this day (that is, fifty-seven years) has not been serious.

When Sir George Stokes, on behalf of the Royal Society, rejected my paper in the year 1868, he did not suggest that it was not serious, and I am sure he never imagined it to be otherwise than serious. His contention was to the effect that it was so much at variance with established principles of natural philosophy that it could not be accepted by any serious scientific society.

Where are now those ' established principles ' of which the Royal Society then declared itself the protecting champion ? Lord Rayleigh and Sir J. J. Thomson have brought

them to issue in their theory of the golf ball's motion, and the gyroscopic pendulum gives a clear and simple refutation to that theory and leaves astral gravitation as the only force which explains the golf ball in the same manner as it explains the spinning top and the circulation of the ocean.

6th June, 1911.

The Royal Society of Arts was prompt in giving a practical refutation to the suggestion ventured in the letter which I addressed to you on 5th ultimo, to the effect that the attack made on Newton's law of gravitation in our Lecture Hall on the 12th May, 1905, would probably be the last battle it would have to fight to maintain its position in natural philosophy.

On the 17th ultimo—that is, only twelve days afterwards, Professor Raoul Pictet, in the Lecture Hall of the Royal Society of Arts, announced that he had just travelled all the way from Berlin at the invitation of Sir Henry Trueman Wood to give us his ideas 'of the world in which we live and suffer and die'—Is there no rejoicing in Berlin?

The attack from New York claimed to 'modify' Newton's law; that from Berlin claims to correct it. Every scientific man is perhaps, consciously or unconsciously, influenced by some dominant idea with which all else in natural philosophy must harmonise or give way. The atomic 'solar systems' alluded to in my letter of 27th August, 1909, in connection with Sir J. J. Thomson's Presidential Address at Winnipeg, seem to have been dominant with Professor Pictet in the Royal Society of Arts. He aims at creating a molecular astronomy as precise and exact as the astronomy of the heavens; and it seems to me that it is solely for the purpose of protecting those precious little atomic systems from being crushed by the reciprocal gravitation of their component parts that he

makes their attraction for 'the ether' greater than for each other at small distances: so Newton's law remains true for the heavens, but has to be 'corrected' when atoms get near each other. 'The ether' in some parts of the lecture looks like astral gravitation, but it is not consistently so. At any rate, if you, Sir William Crookes, would exert yourself to bring science back to the simple common-sense idea of Dalton's atoms as absolute realities, as long as their transitory phases last, then the newly-announced correction of Newton's law would not be wanted. The atom is not only not at variance with Newton's law, but it requires the complete action of that law to enable it to hold itself together. The idea of the evolution of material atoms from immaterial force does not require each atom to be a little 'solar system,' but, on the other hand, the action of gravitation does seem to me to require them to be sturdy particles.

A point in Professor Pictet's lecture which demands attention is his simple definition of inertia as resistance to motion, which has been a guiding principle throughout my published writings for forty-five years. It supplants the 'axiom,' to the effect that matter is indifferent to rest or motion, on which Newton based his first law. Fifty-seven years ago, with perhaps too much feeling of deference to prevalent authority, I ventured to declare that so-called axiom to be really a proposition open to proof or disproof by argument. The only answer which Thomson and Tait, whose text-book became supreme in natural philosophy, deigned to give to that was to the effect that axioms are axiomatic only to those who are competent to understand them. I now welcome the arrival of Professor Pictet to stand with me against Thomson and Tait.[1]

[1] *Raoul Pictet*—19*th May*, 1911, *at the Royal Society of Arts*:

'Aux grandes distances on étudie les phénomènes astronomiques dans lesquels l'éther ne perturbe pas la loi de Newton. Aux distances petites l'éther intervient . . . Ainsi en prenant la théorie de Newton et en lui

15th July, 1911.

I address you for the purpose of protecting myself against being treated as a partisan in the strife which the 'Encyclopædia Britannica' is brewing in connection with the infinitesimal analysis invented more than two hundred years ago by Newton and Leibnitz.

My danger is that the allusion to Newton's Lemma II. in the letter which I addressed to you on 23rd May, 1908, seems to be considered to make me virtually a partisan of those who are now inaugurating a campaign in the Cambridge University Press against Newton's mathematics.

The Ninth and Tenth Editions of the Encyclopædia contain an article of sixty-eight pages on the Infinitesimal Calculus, opening with the words :—'The mathematical and physical sciences owe their present great development to the introduction of the infinitesimal calculus.'

In the Cambridge edition those words have been omitted, together with more than three-quarters of the old article, and the new article (only seventeen pages) not only omits the above panegyric of Newton's infinitesimal analysis, but treats it with (to say the least) scant courtesy, declaring that 'it gained adherents rather through the impressiveness and variety of the results that could be obtained by using it than through the cogency of the arguments by which it was established.'

And further :—'There is also reason to think that Newton had become dissatisfied with the arguments about infinitesimals on which his calculus was based.'

I do beg you, Sir William Crookes, to find out and tell

appliquant les effets de l'attraction de la matière pour l'éther aux faibles distances on reconstitue une immense astronomie moléculaire aussi précise, aussi exacte que l'astronomie astrale.

'Une certaine résistance pour prendre du mouvement, appelée inertie.

'La matière attire la matière suivant la fonction Newtonienne, corrigée pour les distances très petites.'

me what Lord Rayleigh, Chancellor of Cambridge University and Senior Wrangler, says to the idea of a new mathematical process being so complaisant as to yield a 'variety of results.' The idea of Newton having ever been dissatisfied with his calculus, in face of his keen strife with Leibnitz for the glory of priority in its discovery, is also startling news to me ; but I have never professed to be a mathematician, and that feeling of deference to generally accepted authority with which every thoughtful man is, more or less, imbued makes me prefer the position of a keenly interested spectator in the coming fray rather than allow myself to be considered to be a partisan in a question in which I have not attempted to become an expert.

I have not challenged Newton's mathematics. My argument has been to the effect that his Lemmas appear to me to be the origin of the mistake now existing in all astronomical works as regards the orbital velocity of the moon and the corresponding velocities of all the planets and their satellites ; but it is not Newton's mathematics that are responsible for that mistake. The solution obtained by Newton is the only solution that could be given by correct mathematics on the data submitted ; and it is evident that if Newton had submitted correct data his mathematics would certainly have given correct velocities. That is, I think, made clear by my letter of 15th May last.

I venture to suggest that the demonstration I have given in that letter, by using Rankine's definition of potential energy, is the best demonstration of Newton's Law of Gravitation that has ever been given, whilst at the same time it refutes all opponents of Rankine's idea of potential energy.

24th July, 1911.

Where are we now ?

In a Paper read before the English Literary Society

of Buenos Ayres on the 28th July, 1880, I argued that: 'If we admit the practical impossibility of more than partially changing the circulation of India and China from silver to gold, we may safely consider 20*d.* as a practical limit to the fall in silver which the attempt to establish gold monometalism might cause. That price would result from a fall of 50 per cent. in the value of silver, coupled with a rise of 50 per cent. in the value of gold.'

The present price of silver is $24\frac{3}{8}d.$, and does not differ, I think, by more than a small fraction one way or the other from the average price during the last three years, 1908–1910; and in the 'Imperial Asiatic Quarterly Review' for this month—that is, just thirty-one years after my above estimate was made—Sir James Wilson, following the general principles of the argument of my above-mentioned Paper, shows that in the decade just ended—that is, during the years 1900–1910—gold bought on the average 37 per cent. more, and silver 38 per cent. less, than in the decade 1867 to 1877.

As the course of events during the thirty-one years which have elapsed since I gave 20*d.* as the extreme possible fall has been such as to bring the price only to about $24\frac{1}{2}$ as an average for several years, the rise of 37 per cent. in the value of gold, and fall of 38 per cent. in that of silver, now estimated by Sir James Wilson, is not only a very fair corroboration of the principles on which my Paper was based in the year 1880, but also a justification of the views then expressed as to what was likely to be the result of a persistence in demonetising silver among the leading civilised nations.

And what does the estimate arrived at by Sir James Wilson mean in its bearing on our national finances? I will not touch the greater question of industrial finances.

Forty years ago—that is to say, in the year 1871—the

amount of our National Debt was £800,000,000, and it is now about £700,000,000.

The Cambridge edition of the 'Encyclopædia Britannica' says: 'Most States are burdened with enormous quasi-permanent debts, the reduction or extinction of which gives ample scope for the financial skill of statesmen.'

If the financial skill of statesmen has to be credited with the nominal reduction of £100,000,000 of debt in forty years, it must be debited with having created an intrinsic addition equivalent to £259,000,000 as a set-off against the nominal decrease. By legislating in such a manner as to enhance the value of gold 37 per cent. they have made our present debt of £700,000,000 equivalent to £959,000,000 at the value of gold in 1871. In that I do not include anything for the virtual premium at which the £100,000,000 has been paid off.

Besides that, the amount of about £640,000,000 paid in interest has practically given to the bondholders, through the enhanced value of gold, an excess of wealth equivalent to about £200,000,000.

In this position of affairs Sir James Wilson proposes that the Government of India should, after acquiring a sufficient reserve of gold, cancel the notification which binds it to give rupees in exchange for gold, and announce that so long as it has more than £10,000,000 of coined rupees in its possession it will not coin any more rupees. Also that it should at the same time endeavour to supply any demand there may be for sovereigns at all the large Treasuries wherever required in small amounts—'for the more sovereigns there are in circulation, the less is the circulation dependent on rupees'; and though 'it may be some inconvenience to traders and others to be given gold and refused rupees, the hardship cannot be great.'

The gist of that is that any increasing demand for

metallic currency in India must be supplied by gold coins, not by silver coins. And the position therefore is that Sir James Wilson proposes to legislate in such a manner as to force a gold circulation on 300,000,000 of people in India, notwithstanding the clear evidence to the effect that since the principal European Mints and that of the United States were closed against silver the holders of our National Debt have received at least the equivalent of £200,000,000 more of the national wealth than they would have received if gold had remained at the same value as in the year 1871, and the fact that our present debt of £700,000,000 is equivalent to a debt of £959,000,000 at the value of sovereigns in the year 1871. Not only is the gold now being forced on India *not* wanted there, but it is all very much wanted here.

Let it be granted that Lord Herschell's Committee were right in the policy of closing the Mint in 1893 temporarily against the coinage of rupees; that policy would be at least as effective against depreciation without at the same time forcing gold into the country, and it would have left silver some chance of rising again to its rupee value with increase of wealth and population.

But all the demand created for gold in India thwarts an equivalent demand for silver, and has the double action of enhancing the value of gold and depreciating that of silver, thus destroying any prospect of a recovery of the position of silver.

Sir James Wilson tells us that since the sovereign was made legal tender in India, in the year 1899, India has absorbed £82,000,000; and out of that amount £35,000,000 has been absorbed during the last two years, showing an increasing rate of absorption. It seems to me that the flow of gold to India artificially created by Indian legislation is thwarting a demand for an equivalent value of silver, and is making the London quotation for silver lower than

it would otherwise be. But Sir James Wilson seems to be quite unconcerned with the effect of the enhanced value of gold in this country. It is his business to consider the finances of India. But in fact the 'cross of gold' which the taxpayer in this country is now compelled to carry has become heavy for even his broad shoulders, and the supply of gold which would facilitate his payments ought not to be artificially forced into other directions to his detriment.

On the 18th April, 1883, Mr. Goschen read a Paper before the Bankers' Institute expressing views so much in accordance with the principles explained in a Paper on 'Lord Liverpool's Oversight,' which I had read before the English Literary Society of Buenos Ayres on the 14th June, 1882, that I expected a speedy restoration of the Double Standard in London under his guidance; but he relapsed, and in fact distracted the attention of the City from that serious question by the display of financial fireworks which has been chiefly responsible for bringing the quotation for Consols down to 78⅜. The City's indulgence in that short-sighted legerdemain just when the legislation provided for by the Lords' amendment to the Currency Bill of 1816 was required, as foreseen in the House of Lords and immediately recognised by the Commons, may before long become the greatest disaster this country has ever encountered.

The restoration of the Double Standard was easy at the time Mr. Goschen was playing pranks with Consols instead of dealing with it. It has now become less easy, but at the same time more imperative.

28th July, 1911.

The article on 'Money' in the Cambridge edition of the 'Encyclopædia Britannica' contains a mistake which may lead to pernicious consequences if allowed to stand uncorrected in so influential a publication.

It is there complacently stated that :—'The full legal establishment of the present metallic currency took place in 1816, when the guinea made way for the present pound or sovereign, and silver was formally reduced to the level of a token coinage.'

In the first edition of my work 'The Standard of Value' I made that same mistake, saying :—'In 1816 the debt amounted to 850,000,000 pounds sterling, each of which was, by the terms on which it was borrowed, either 1,718 grains of silver or 113 grains of gold, at the option of the debtor ; but Parliament then decreed that the debt which had been borrowed with the right of payment in silver must be paid in gold ; for they closed the Mint against silver, except for the coinage of a limited amount issued at an enhanced value for small change, and declared gold the only legal tender for the pound sterling ; the Mint being open for the unlimited coinage of the gold sovereign.' I was not, however, complacent with that historical statement, as the Encyclopædia seems to be. I urged that : 'the chief representatives of the bondholders are bankers, merchants, and capitalists, who have sufficient knowledge and influence to protect their just interest ; whereas the chief representatives of the debtors are labouring in the fields, in the factories, or in the mines ; and as the Government of the day either had not the patriotism or power to defend their just interest, or else did not properly understand the influence of the measures which deprived them of the right to pay in silver, the debtors have been defrauded by Act of Parliament.' And I further suggested that :—'I do not for a moment suppose that the majority in Parliament passed that measure with the wilful intention of defrauding the debtors.' 'It was doubtless at that time expedient for gold, rather than silver, to be coined ; but nevertheless the Mint should have been allowed the right of purchasing silver for

coinage whenever it could do so at a fair profit, as in fact it could do at present.'

Since the above suggestion as to the mistake of omission in the Currency Act of 1816 was made by me before the English Literary Society of Buenos Ayres on 28th July, 1880, the 'Bankers' Magazine' has ascertained that the Currency Act of 1816 does actually contain such a clause as I then suggested it ought to have contained. I have not seen the Act, but understand from the numerous comments which followed the startling and important statement made by the 'Bankers' Magazine' that in effect it authorises the King in Council, without further intervention of Parliament, to order the Mint to resume unlimited coinage of silver for the public if at any time equity was considered to require such a course.

The Currency Act of 1816, therefore, in fact, did *no* establish the present metallic currency as asserted by the 'Encyclopædia Britannica.' That Act provided for the unlimited coinage of silver as well as of gold. It was not until the year 1870, when Mr. Robert Lowe was Chancellor of the Exchequer, that a new Currency Act was passed in substitution for that of 1816, foolishly suppressing the wise and equitable provision for the maintenance of the Double Standard contained in the parliamentary legislation of 1816.

I do not know the precise terms of either Act, but the information given by the 'Bankers' Magazine' and other reliable journals was well known to everyone who took any interest whatever in the subject, and it is amazing that after its appearance so important a publication as the Cambridge edition of the 'Encyclopædia Britannica' should repeat in the year 1911 the mistake which I made in Buenos Ayres in the year 1880 (thirty-one years ago). Courtesy requires me to suppose that the Article on 'Money' was

written for the Encyclopædia in ignorance of the facts of the case; but the bare idea of the alternative being possible leads me to beg Cambridge, of which our Institution has virtually become an appanage, to reflect on the words: 'In honour shall a kingdom be established.' We are face to face with the most important financial question the civilised world has ever had to deal with, and it is deplorable that the influence of Cambridge University should be brought to bear on it under the guidance of false ideas as to important matters of fact.

3rd August, 1911.

With the evidence we have before us to the effect that the new supplies of gold have not been sufficient for the legitimate demands of gold standard countries, as recorded in my letters of 24th and 28th July, it is to me appalling to read the propaganda now in full cry in the Cambridge edition of the 'Encyclopædia Britannica' and in Sir James Wilson's Paper on Indian Currency, virtually endeavouring to induce seven or eight hundred millions of people in India and China to become dissatisfied with their silver currency and to compete for the supplies of gold which are clearly insufficient even for the now existing demand.

The question is made urgent by Sir James Wilson's proposal to coin sovereigns and ten-rupee gold coins in India.

There can, I think, be no difference of opinion first, as to the ease with which such a course can be made effective, the Mint being closed against silver, gold will be offered for coinage to supply all increased demands for currency. Secondly, as to the tendency of that action by the Mint to increase the existing difference in the relative value of silver and gold. And thirdly, as to the Indian legislation against silver and in favour of gold being sure to lead to a demand for gold currency in China.

I repeat, our National Debt of £800,000,000 in 1871 has been nominally reduced to £700,000,000, but its tax on the wealth of the country is £159,000,000 greater than in 1871, though we have paid off in hard cash one hundred million pounds of the capital.

I refrain from repeating arguments already published which led me to write on 18th July, 1882, from Buenos Ayres—'I am convinced that the influence of the enhancement of the standard of value in contributing to the fall of the Roman Empire by making the rich more rich and the poor more poor, thus leading to reckless extravagance on the one hand and to the recklessness of despair on the other, has never hitherto been fully appreciated; and that the evil effects of the recent enhancement of the standard, together with the greater evils that may yet arise from it, have not been overestimated by Ernest Seyd, Cernuschi, Wolowski, or Laveleye.'

The latter part of Mr. Goschen's Paper read before the Bankers' Institute on 18th April, 1883, elaborately confirms that view, and gives a practical warning of the disasters which must result from allowing gold to be called on to supply too great a part of the world's demands for currency.

8th August, 1911.

I find, on looking into records of the year 1816, that the relative positions of Lords and Commons in opposing Lord Liverpool's attempt to abolish the Double Standard is not technically correct as given in my letter of 24th July, on authority which I no longer have at hand.

What seems to me to explain the mistaken position there recorded is that the leading champion of the silver standard was Lord Lauderdale, a member of the House of Lords. He failed in the first instance in his attempt to

prevent Lord Liverpool's Government from passing their Gold Standard Bill through the House of Lords. But two days afterwards, in the House of Commons, the Government themselves introduced an amendment to the effect that the Mint might at any time recommence the unlimited coinage of silver for the public under any order of the King in Council without further reference to Parliament. The Bill, so amended, was returned to the Lords, where it passed its third reading on 7th June, 1816, and became the law of the land.

When Mr. Gladstone's new Currency Bill came before the Lords on the 23rd March, 1870, the Duke of Richmond pointed out that as originally drawn the Bill provided that silver as well as gold brought to the Mint should be assayed and coined, but silver had been struck out; and he asked to be informed for what reason it had been struck out. The Marquis of Lansdowne (now opposition leader in the Lords), in replying for the Government said: 'With regard to the omission of silver, the Government make money by coining silver and not by coining gold, and therefore deemed it advisable not to include silver in the provisions of the Bill.'

That conclusion, or, at any rate, concluding argument, carried the Bill through its third reading in the Lords, making gold the sole legal standard of value in this country, on 25th March, 1870.

The Cambridge edition of the 'Encyclopædia Britannica' ought to know better than to teach that gold was made the legal standard of the country in the year 1816.

The *Times*, in leading articles on two successive days about 31st May, 1816, ridiculed Lord Liverpool's contention that gold had become the standard of value in the estimation of the people; and seems to me very neatly to justify the argument in my work on 'The Standard of Value' to the

effect that his treatment of the question was not that of a statesman bent on impartial justice between debtor and creditor, but that of a partisan of the bondholders bent on extorting the utmost farthing from the taxpayers.

The same may, in the aggregate, be said of those who domineered the Paris Conference of 1867 and led Mr. Gladstone's Government to discard our Double Standard three years later.

12th August, 1911.

The Cambridge edition of the 'Encyclopædia Britannica,' in the article on 'Geometry,' tells us: 'It is admitted on all hands that in practice only relative motion is measurable.' And, after referring to Newton in a manner which shows Professor Whitehead to have misunderstood the definition of absolute motion given in the 'Principia,' says: 'This irrelevance of absolute motion (if there be such a thing) to science has led to the general adoption of the relational theory by modern men of science.'

That is the answer which comes from Cambridge to the demonstration of the absolute velocity of the moon's orbital motion which I have given in some of 'The Sling Letters' I have addressed to you!

Lord Rayleigh, Sir Joseph Thomson, and Sir James Dewar, your three Professors of Natural Philosophy whom I have challenged on the question of some vagaries of the golf ball's motion, are all joint authors of the Encyclopædia with Professor Whitehead. They have a perfect right to their views of absolute and relative motion; but as, in fact, my use of the term *absolute motion* is exactly in accordance with Newton's definition, they have no right to declare it to be admitted on all hands that in practice only relative motion is directly measurable.

Is Newton nobody? Professor Whitehead's misunder-

standing of Newton is very much the same as that of Professor W. H. Macaulay in the Encyclopædia's article on 'The Laws of Motion,' which I have explicitly dealt with in my letters of 8th and 15th May last.

19th August, 1911.

The Chronological Section of Professor C. H. Turner's article on 'The Bible' in the Cambridge edition of the 'Encyclopædia Britannica' is far from up-to-date.

In the year 1900 I offered you (the R. I.) a short Paper on Chronology, showing that what was long clearly recognised to be the first year of the Christian Era is now erroneously treated as 1 B.C. by historians. The Encyclopædia discusses disputed dates in the early days of the era without any apparent knowledge of that simple fact, and therefore cannot *prima facie* be relied on as a quite safe guide.

Whilst my Paper was passing through the press in 1904 I added two calendars, one for the 'Era of Christ,' which seems to have been used in England long before the arrival of St. Augustine with the Dionysian calendar; and the other for the 'Era of the Crucifixion,' which seems to have been in use in some Eastern countries at the same time.

It is reasonable to suppose, in the absence of evidence to the contrary, that the English calendar was based on the true date of the Nativity, and the other on the true date of the Crucifixion; and until it is shown that reliable evidence cannot be obtained from them, they ought not to be ignored in such an elaborate discussion of those dates as is given in the Cambridge Encyclopædia.

I avowedly gave the calendar for the Crucifixion tentatively, and am not confident as to the treatment of either of the calendars in my work or elsewhere.

Roughly speaking, Professor Turner argues from the

same data as J. J. Bond, but arrives at a different result as regards each of the dates, so that the clashing of the arguments makes them, as far as the general reader is concerned, mutually destructive.

23rd August, 1911.

Dr. Otto Krummel, of Kiel University, Lecturer in the German Imperial Naval Academy, in conjunction with Dr. Hugh Robert Mill, has favoured the Cambridge University Press with an article on 'Oceanography' for their edition of the 'Encyclopædia Britannica.'

In the Section on 'Ocean Circulation' Cambridge University tells us that 'the general scheme of ocean currents depends on the prevailing winds, taken in conjunction with the configuration of the coast and its submarine approaches.'

In reply to that it might be sufficient to remind Cambridge of the work in which Captain Maury, more than sixty years ago, tore to tatters that idea of the winds being the cause of the existing circulation of the ocean, especially as the absence of clearly specified detail or of any chart to support the views expressed in the Cambridge article makes practical argument not quite readily brought to bear on its plausible theorising; but I will, however, quote as a specimen of plausible theorising the statement that 'The trade-wind regions correspond pretty closely with westward-flowing currents, while in the equatorial calm belts there are eastward-running counter-currents.'

That reads very well to anyone who does not know what the actual facts of the circulation are. One of Maury's arguments was to the effect that, as the Trade Winds converge from N.E. and S.E., any surface water swept along with them would tend to create counter-currents in the Calm Belt, but that in fact in those regions the current

often runs *westwards* as strongly as in any part of the Trade-Wind regions.

Not only are the equatorial currents found running westwards as strongly in the Calm Belt as in the Trade-Wind regions, but also their counter-currents are found running *eastwards* right against the Trade Wind as strongly as in the Calm Belt. The evidence given by Captain Maury to the effect that the winds are not the cause of the equatorial currents and counter-currents has been abundantly confirmed by A. G. Findlay's more recent Sailing Directions.

Not only is the suggestion that eastward-running counter-currents are characteristic of the Calm Belt at variance with what has become well known to all students of the subject, but besides that disregard of ascertained facts, the Cambridge article also ignores the evidence which has made it clear that surface currents diverge from the equatorial currents, first crossing the Trade Winds and then curving eastwards right against them in the central parts of each of the great oceans. That is now clearly shown by records in Findlay's Sailing Directions.

The authorities of Kiel University may think it a good joke for Dr. Krummel to have planted obsolete views of wind action in Cambridge—an inland University, but I scarcely think he would venture to offer them for home consumption in his own Imperial Naval Academy.

25th August, 1912.

After dominating Tidal Theory in the Royal Society for seventy-five years, Dr. Whewell's Chart of Tide Waves has been scrapped in the Cambridge University Press.

I gave that chart a gentle tap from a purely theoretical point of view in the year 1866; and two years later I not only elaborated the theoretical argument against it, but

also showed that facts by which it had been supported had been misinterpreted.

The simplest feature of misinterpretation was that similar tide hours on opposite sides of the South Atlantic were treated by Dr. Whewell as extremities of the same tide wave flowing northwards, whereas in fact they are independent waves, moving westwards in the equatorial regions and eastwards in the temperate zone, and then deflected by the coast lines. And, besides that, I showed that records had accumulated in the Admiralty Tide Tables in such a manner as to give it direct refutation. It has been rickety ever since; and now, in the article 'Tide' in the 'Encyclopædia Britannica,' Sir George Darwin says: 'We think it more prudent to refrain from reproducing it again.'

Tollitur Quaestio

As the great tide wave flowing from the Southern Ocean to our shores in $1\frac{1}{2}$ to $2\frac{1}{2}$ days has vanished, we are faced with the question as to why the high spring tides in our ports do in fact occur about two days after conjunction of the sun and moon instead of at conjunction.

The explanation has been that the tide waves are raised in the Great Southern Ocean and take that time to reach our ports. Supposing the existence of such waves to have been disproved by the facts which I published in the year 1868, I that same year offered to the Royal Society a Paper showing that, in consequence of the difference in the distances of the sun and moon from the earth, their tidal action is not in conjunction until about two days after they have been in conjunction or in opposition.

That explanation now holds the field, as its only rival is gone.

28th February, 1912.

Soon after nine o'clock last Friday evening our Lecture Hall was darkened and on the large white screen there was made to appear the ghost of Newton's First Law of Motion, which was ridden to death by Sir William White and his comrades in naval construction some years ago. That poor ghost was summoned to the lecture for the express purpose of being flogged into an explanation of the Gyrostatic Compass. As far as I was concerned, the compass itself remained as inscrutable throughout the lecture as the Veiled Prophet of Khorasan, but gyroscopic experiments were shown in which resistance to change of plane of rotation could quite plausibly be attributed to the action of Newton's law. Throughout about three-quarters of an hour there was, however, in each experiment one direction for change of plane in which such change was prevented by the mechanism of the gyroscope; so that if it were granted that Newton's law gave the true explanation for the plane not changing in the direction in which it was not mechanically restrained, the question arose as to whether that law would avail to prevent change of the plane if not mechanically prevented in the other direction.

A large gyroscope which had been hanging by a long cord from the ceiling was at length approached by the lecturer, who was evidently conscious of it being the *pièce de résistance* of the evening. It was the first experiment in which the gyroscope was free to change its plane in all directions.

The scene we then had before us so nearly approaches the incredible that I dread describing it, at the risk of being considered a romancer. The gyroscope was rotating in a north-and-south plane. The lecturer attached to one side of it a weight in the manner which had failed to cause change of plane in previous experiments. He then

triumphantly called on us to witness to the manner in which Newton's law was vindicated by the gyroscope *refusing to change its plane of rotation.*

The glaring fact, however, was that the instant the weight was attached to the gyroscope it began to change its plane of rotation, and in less than a minute it was rotating in an east-and-west plane instead of its previous north-and-south plane, the lecturer all the time calling on us to notice how it vindicated Newton's law by *not* changing its plane.

The reason why, in the experiment with the model of the Brennan monorail car, a similar action did not cause a change of plane, was simply because the pressure of the flanges of the wheels against the rail prevented the car from turning round so as to change the gyroscope's plane of rotation ; and the evidence given by the hanging gyroscope to the effect that the mechanical action of the rail—not Newton's law—keeps the car from turning round, makes it absurd to suppose that Newton's law gives the true explanation of the monorail car not capsizing.

The hanging gyroscope experiment was applauded through a confusion of ideas. Though the audience had just seen that the weight did not capsize the monorail car they were evidently watching for a capsizing movement of the hanging gyroscope instead of the turning motion which the rail prevented the Brennan car from making.

The key to the explanation of gyroscopic action was first given on 3rd November 1877, in a lecture which I delivered in Willis's Rooms and afterwards extended in my ' Essays on the Action of Astral Gravitation in Natural Phenomena.' The position of the controlling stress in gyroscopic action was unknown until I gave it in the above-mentioned lecture ; nor, so far as I know, had it ever before

been suggested that the gyroscope is controlled by the reaction of vis inertiæ against its motion—not by the direct action of the motion. That in fact cannot logically be admitted in science without discarding Newton's definition of vis inertiæ and substituting for it a recognition of the simple fact that inertia is merely the opposite aspect of universal gravitation as shown forty-four years ago in my 'Treatise on the Action of Vis Inertiæ in the Ocean.'

31st March, 1912.

Professor Bertram Hopkinson of Cambridge began his interesting lecture on Friday evening, the 26th January, by telling us that it was so long since many of us had left school that we perhaps might not remember accurately Newton's laws of motion; and that, as his arguments throughout the lecture would be based on them, he thought it desirable to commence by giving them to us in a short explanatory statement.

In view of the manner in which those laws have been treated in the Cambridge edition of the 'Encyclopædia Britannica' and the criticisms I have addressed to you, I was perhaps more eager than anyone else present to hear the Cambridge Professor's explanatory statement of them, but I was quickly disappointed by his plunging into the theory of projectiles without any shadow of the promised statement; and, in fact, the most important point of Newton's first law of motion which I attacked and scrapped some years before Professor Hopkinson was born was completely ignored by him. It was not involved in any of the explanations he gave. As far as that first law is concerned he was guided, not by Newton's law, but by the corrected form given to it in the works I published before the young Cambridge Professor was born. It seemed to me at times

that the lecturer was better acquainted with my works than with Newton's. Be that, however, as it may, in the latter part of the lecture he apologised to the Royal Institution, with Lord Rayleigh in the chair, for venturing before so distinguished an audience on a new explanation of the action of the cap which is now always placed over the front of heavy projectiles. Knowing what I know in connection with that subject, it was not possible for me to be otherwise than on the *qui vive* to hear the explanation which I gave on the 10th February 1903, in the Institution of Civil Engineers, repeated to the Royal Institution, as the repeated apologies for what was about to be said seemed to preclude the generally accepted explanation and I can imagine no alternative but that which I gave in the discussion in the Institution of Civil Engineers as recorded on pages 47 to 49 of Vol. CLIII. of their proceedings. But, notwithstanding the flourishes of trumpet with which it was heralded, I could not see that the explanation given differs intrinsically from what I have always understood to have been generally received. It was impossible for Lord Rayleigh to object to the application of the simple term *inertia* to the action of the cap in that explanation seeing that the lecturer had refrained from using it in the sense of momentum. But, using the terms in the same sense as Professor Hopkinson, I say the momentum, not the inertia of the cap makes the plate vibrate and so weakens its resistance before the main blow of the shell reaches it. Unless the lecturer was playing fast and loose with the meaning of terms, what he gave us was the old explanation and not a new one.

I now pass on to a more important part of the lecture.

Perhaps Professor Hopkinson is right in suggesting that the long time elapsed since I left school may have resulted in my forgetting Newton's treatment of some subjects.

It certainly seems to me strange that Professor Hopkinson should apply Newton's methods to *relative* instead of to absolute motions of projectiles. It is true that Newton dealt only with relative motion in the heavens. But that was simply because he never succeeded in finding a basis for determining absolute motion beyond the range of terrestrial phenomena. His analysis of the forces exerted by bodies in motion was based on absolute motions in practical experiments; and his elaborate dissertation on the manner of distinguishing between absolute and merely relative motions seems to me to show one of his great ambitions to have been to find a basis for determining in the heavens absolute velocities of motion as defined by him. His misunderstanding of the ratio of forces involved in the action of the Sling made it impossible for him to distinguish absolute motion in the heavens; but, after discovering the true ratio of those forces, I was able to show, in the letter I addressed to you on 15th May of last year, that Rankine's law of potential energy combined with Newton's law of gravitation in the motions of the planets exactly as it does in the motions of projectiles, so giving the velocities of absolute motions of the moon and planets in the sense in which Newton defined it in terrestrial phenomena. The figures I have given in the above-mentioned letter make it clear that both in astronomy and in terrestrial physics absolute velocity is as the square root of motive force and resistance as the square of velocity under similar conditions. In the heavens that is shown by Newton's law of gravitation combined with Rankine's law of potential energy to be the basis of the action of physical forces above stated; and in terrestrial phenomena it is confirmed by Newton's numerous experiments with pendulums. Some of the arguments and figures given by Professor Hopkinson seem irreconcilable with that basis.

For instance, on page 13 of the printed paper issued a few days ago he treats the pressure of two bodies meeting at a relative velocity of 2,000 feet per second as a fixed quantity not affected by what their respective absolute velocities may be. But, in fact, if one body be at rest and the other moving with the absolute velocity of 2,000 feet per second the pressure is represented by $2{,}000^2 = 4{,}000{,}000$; whereas if the two bodies meet, each with a velocity of 1,000 feet, the pressure is then represented by $1{,}000^2 + 1{,}000^2 = 2{,}000{,}000$. That is to say, if the relative velocity of 2,000 feet is due to equal absolute velocities in the two bodies the pressure is only half what would be created by the absolute velocity being all in one of the bodies.

It is dangerous in physics to base arguments on anything but absolute quantities, whether of matter or of motion.

5th May, 1912.

In a letter to the Royal Institution on 27th August, 1909, I diagnosed that the address delivered by the President of the British Association at Winnipeg showed the atomic solar systems which had for many years been the favourite pets of science to be in danger of abandonment by their most renowned supporter. The series of Saturday afternoon lectures recently delivered in our Lecture Hall by Sir Joseph Thomson constitute a complete justification of the view I then took as to the direction in which his ideas were trending.

Not only were the precious little things never once mentioned throughout the six lectures, but arguments were used which were quite incompatible with the existence of the poor little flimsies. Sir Joseph Thomson seemed to be deliberately searching in the atom for evidence of transition from immaterial force to matter, or from matter

to immaterial force—I could not make out which. Success in either direction would be a step towards establishing the idea of evanescence which I got hold of about the year 1852, leading immediately to the action of astral gravitation and the abolition of Newton's so-called first law of motion.

As President of the British Association in South Africa in 1895 Sir George Darwin, without using the term evanescence, attributed the idea to Heraclitus, and I for some time looked forward to further information regarding the long-forgotten opinions of that ancient philosopher in face of the world-wide fashion in which he was proclaimed the originator of what had become the basis of modern science.

What is certain is that at the time I published my first work in 1866 scientific opinion was all in favour of the idea of the indestructibility of matter ; and I venture to suggest, Sir William Crookes, that you might very well invite Sir George Darwin to give us a lecture on Heraclitus so as to make the origin of the idea of material evanescence as opposed to indestructibility less vague than it stands at present.

I am, dear Sir,
Yours truly,
Wm. Leighton Jordan.

Letters to His Majesty's Most Honourable Lords Commissioners, the Admiralty

ROYAL SOCIETIES CLUB,
St. James's Street, S.W.,
22*nd September*, 1911.

My Lords,—Captain Blunt used the 'Hawke' in the Solent on 20th instant for a practical though expensive and dangerous experiment for illustrating tidal action which I have long been pressing on the attention of Scientific Institutions and on the Admiralty.

In my 'Sling Letters' (many of which are addressed to your Lordships) I find on page 199 of Part III. I have said :—

When a steamer of 30,000 tons is crossing the ocean at a speed of twenty knots an hour the first effect on the pathway traversed by the vessel is not where her prow treads the water, but a long way in advance of that position ; and that first effect is not a piling up of the water by the pressure of the ship's motion, but a *sinking of the ocean level* to form a tidal wave with its crest astern of the ship, so as to preserve the balance of gravitation disturbed by the ship's motion.

As long as Captain Blunt was going ahead to pass the 'Olympic' he was all right ; but, immediately he slowed down, his ship became the most readily available mass for the earth's gravitation to pull into the position vacated by the 'Olympic.' The force of gravitation exerted to bring his ship under the stern of the 'Olympic' is represented by the greater part of the horse power exerted to drive the latter ship at her speed of 15 knots.

25th September, 1911.

In my letter of 22nd instant, of which I have this morning received your Lordships' kind acknowledgment, I notice that I have by inadvertence said 'represented by the greater part of the horse power.' What I had before me at the time of writing was the greater part of the reaction on the starboard side of the 'Olympic,' which would be *one-eighth* of the horse power being exerted by the 'Olympic,' and even that, though perhaps not far from correct, must be taken as an assertion to the effect that a very great force of gravitation not hitherto taken into consideration in such matters was acting *directly* on the 'Hawke,' pulling her out of her intended course when the accident occurred.

2nd November, 1911.

I hope your Lordships are not allowing the promise of attention to the subject of my letter of 25th September to be forgotten.

The principle involved in the action by which the 'Hawke' was pulled against the 'Olympic' is the same as that which has been misunderstood in connection with the main features of tidal action in the ocean.

It is now not far from forty years since Sir George Nares, on taking command of the 'Challenger,' received a special instruction from your Lordships' predecessors in Office to ascertain and report to them as to the course of the tide waves of the Great Southern Ocean. His withdrawal from that command before completion of the programme I have always regarded as a great disaster to the cause of oceanic exploration. The question as to the course of the tide waves in the Southern Ocean is only one of the questions thrown adrift by the change of command.

As the collision of the 'Hawke' and 'Olympic' in the

Solent gives practical importance to the subject, a concise statement of the tidal action seems expedient at this moment to show how the action I have described in connection with the motion of the 'Olympic' corresponds with that which governs the tide waves created by the moon's tidal action.

My contention is that by action precisely similar to that by which a mass of water equivalent in weight to the 'Olympic' is drawn back to restore the equilibrium of gravitation so also as the direct action of the moon's gravitation carries a tidal wave along with the moon's motion over the equatorial regions, the earth's gravitation preserves the equilibrium on the earth's surface by carrying at the same time in the opposite direction through the two Temperate Zones an equivalent mass of water to counter-balance the weight of the equatorial tide just as the moving mass of the ship is counter-balanced by an equivalent tidal action in the opposite direction.

Have any of your officers, My Lords, complied with the instruction given to Sir George Nares? If the question has been answered why has not the answer been published? And if it has not been answered why is so important an instruction left in abeyance?

That tidal question was given so prominent a position among the objects which the 'Challenger' Expedition was appointed to solve that it must be regarded as representing an important part of the expense and trouble incurred.

25th November, 1911.

We live and learn.

I have just read in to-day's *Times* yesterday's proceedings in the Hawke-Olympic trial, and I refrain from saying anything more about the complications of suction except that it seems to me that the letters I addressed to

the Institution of Civil Engineers on 22nd and 25th March, 1905, and afterwards published in Part II. of 'The Sling' have evidently had the effect of leading to practical experiments on the subject.

I write now because this is an opportune moment for calling attention to quite a different dynamic action which seems to have been overlooked in the Hawke-Olympic discussion as reported in the *Times*.

Between Southampton Water and the Bramble Buoy the 'Olympic' accumulated southward momentum which would all have to be expended in an effort to give *starboard way*, not headway, after she turned on her course of S. 59 E. The slant of starboard pressure would result in a great part having effective action as headway, the ship not having turned as much as a right angle, but her course would have to sagg more or less to starboard.

The arguments, however, regarding suction as reported in the *Times* suppose that the ship on being headed S. 59 E. would normally make a straight course in that line.

That starboard way might account for Captain Blunt's assertion that the 'Olympic' was leaving him too little room when trying to pass on her starboard side. The corresponding action northwards on the 'Hawke' would be less in proportion to the wider angle at which she turned Egypt Point.

That persistence of momentum in the direction of its creation can be made strikingly apparent in pendulum experiments, according to which if the 'Olympic' had turned ninety degrees at the Bramble Buoy and whilst doing so stopped her engines all her momentum would have been expended in starboard way leaving her no headway.

I do not think that any scientific man on having his attention called to that action will dispute it. In fact it appears that the ships were closing in on each other under

the action of their own momentum, with which the action of suction combined when they were already closer than either Captain perhaps intended.

27th November, 1911.

I notice that from one part of my letter of 25th instant, it might be inferred that I had never myself made any such experiments as those referred to in the Hawke-Olympic trial. I think it expedient to correct any such impression, which I can do without involving any wrangle about priority as what I did had doubtless often been done long before, the difference merely being that I experimented for the express purpose of testing the action of the waves I have since depicted on Figure 3, page 135, of Part II., whilst it had previously been done perhaps only for fun or bravado.

Somewhere about forty years ago in the delta of the Paraná I selected a suitable spot which a steamer had to pass. I pulled a pair of sculls in a long, light boat. On joining the steamer's course about abeam I found I had to pull all on one hand at a time, first one and then suddenly the other. Between that and the shrieking of the steamer's whistle I failed to get on the crest of the wave but shot over it and had to pull hard to escape being wrecked against the steamer's rudder.

A week or two afterwards I tried again and easily got on the crest of the wave on the starboard quarter of the steamer, where I had to pull with the right hand only to keep that position. I kept pace with the steamer, or rather gained on her, until, as I was gradually getting near the paddle wheel and the steamer screamed more and more, I backed water with my left hand whilst still pulling with the right and had to make a considerable effort in that

manner before the boat responded and turned with a jerk [1] so as to leave me pulling right away aft from the steamer. She dipped her flag and I stopped to wave my hat in response to that and the Captain's salutation from the bridge. So that time I parted on good terms.

I have the honour to be, my Lords,
Your most obedient, humble Servant,
WM. LEIGHTON JORDAN.

[1] The jerk seemed partly due to the suction pressure being suddenly transferred from the starboard to the port bow as the boat turned away from the steamer, and partly to the pressure of the propelling wave, which had acted against the middle of the port side, being transferred to the starboard quarter. Before turning, the bow was dipping over the wave so that the stern was somewhat lifted. The deeper draught of the 'Hawke' would make the action of that wave relatively less important than the suction, but she must first have had the wave pressure against her port *bow*, counteracting the suction, and then as she came abeam of the 'Olympic' she would get it on her port *quarter*, thus suddenly acting with instead of against the suction to turn her head to port. When I forced my boat's head to starboard I got that pressure on the *starboard* side.

The pressure of that wave seems also to have been overlooked in the Hawke-Olympic trial.—W. L. J.

Letters to Dr. J. H. T. Tudsbery

July 1911 *to April* 1912

ROYAL SOCIETIES CLUB,
St. James's Street, S.W.,
10*th July*, 1911.

Dear Sir,—An article in to-day's *Times* on the retirement of Sir Philip Watts from the post of Chief Constructor of the Navy brings to mind an interesting aspect of the recent history of science in connection with shipbuilding.

On 12th January, 1903, I addressed a letter to you for the purpose of drawing attention to erroneous principles by which our Naval Constructors were then guided. The presence of Sir William White on our Council seemed to make our Institution the most suitable body I could address with a feeling to the effect that what I wrote ought to be treated as deserving of consideration. Unfortunately Sir William White was evidently imbued with the same idea which led the Editor of 'The Engineer' to tell me that if he admitted into his paper any question as to the truth of Newton's First Law of Motion he would make himself the laughing-stock of the engineering profession, and he did not allow me an opportunity of discussing the subject with him.

Sir William White about that time withdrew from the post of Chief Constructor, and was succeeded by Sir Philip Watts, who, little more than two years after the date of my above-mentioned letter, laid down the keel of the notorious 'Dreadnought' to be built on lines greatly differing from

Sir William White's ships, and in the manner required by my arguments.

A great official authority tells us that: 'Sir Philip Watts must bear on his broad shoulders all the praise or all the blame which was to be associated with the name 'Dreadnought.' That—viewed as a question of national policy—the building of that ship must be regarded as a disastrous blunder is, I think, now generally admitted; but it is the Civil Lords of the Admiralty, not the Naval Constructors, who are responsible for that. And as for the superiority of the 'Dreadnought' as a fighting ship, I understand that Sir Philip Watts has over and over again disclaimed any personal credit for that superiority except as having been one member of a Committee by whom the expediency of changing from the shape of the 'King Edward VII.' was discussed. I venture to say that never before in the annals of shipbuilding has so much modesty been shown by those concerned as regards claiming the chief merit for so splendid a success.[1]

I hope some member of our Council may be led by the foregoing to reflect on the eight years which may be termed the 'Dreadnought' era of construction, and recognise that the Council have been mistaken in disregarding some of my contributions.

11th November, 1911.

Mr. Halligan's reply in Vol. CLXXXV. in the discussion of his paper on New South Wales Harbours throws into bold relief the error of our 'Publication Committee' in excluding from the published report of the discussion my argument as to the difference in the action of the inertia of the ocean water on the east and on the west coast of Australia.

The discussion of the effect of ocean currents, or tidal

[1] Whilst writing the above I forgot for the moment the explicit character of the claim made in some of the letters I have addressed to you.

currents, on the harbour bars of the Australian coast without either side in the discussion recognising that on the west coast there is a general tendency for the undercurrents to rise against the coast and pile sand against it, whilst on the east coast the tendency is for the *surface* water, which is not so often sand-bearing, to press against the coast and drive the undercurrent from the coast—a discussion of that subject so conducted by a group of engineers places them on all fours with a company of players acting or trying to act 'Hamlet' without the Prince of Denmark. 'The Action of Vis Inertiæ in the Ocean' is as important a consideration in the treatment of harbour bars as is the Prince of Denmark in the performance of 'Hamlet.'

I am not disposed to be authoritative as regards what may be the predominant action at any harbour bar without special knowledge of the locality, but whilst engineers are in disagreement as to whether other possible actions are or are not predominant, it seems to be mere matter of fact that the normally open bar at Sydney, on the east coast, compared with the sand-encumbered bar at Fremantle, on the west coast in the same latitude, may reasonably be regarded as *prima facie* evidence of the action which has been refused a place in our published Proceedings.

13th November, 1911.

The subject of my short letter of 11th instant demands a broader view of its surroundings. My remarks on Winds and Currents in Vol. CLXXXIV. of our Proceedings were all very well as supplemental to my argument on the action of oceanic inertia, but standing alone they want more *raison d'être*.

You know that the idea of the inertia of matter making it indifferent to rest or motion was invented by Galileo

for the purpose of explaining the continuity of the earth's motion. A leading member of the Royal Society, assuming that definition of inertia to be unquestionable, recently contended that the common sense of anyone who rightly comprehends the fundamental principles of physics tells him that the ocean cannot tend to lag against the earth's rotation.

In a paper which I submitted to the Council of the Royal Geographical Society in the year 1866 I gave a chart showing the circulation which must naturally be caused by the earth's rotation if the inertia of the ocean causes it to tend to lag against that rotation—that is to say, supposing Galileo's definition to be incorrect.

As a matter of fact, Mr. Halligan's sketch of the ocean currents in the vicinity of the Australian coasts agrees exactly with my old chart on all the four coasts—north, south, east and west. Both charts make the westward current of the north coast as important as the southward current on the east coast or the northward current on the west coast.

But, some time after the publication of my 1866 chart, I found that in theory the vertical action of the lagging bears a much greater proportion to the horizontal action than I supposed when drawing that chart ; and the consequence is that much of the water I supposed to pass westwards from the Pacific to the Indian Ocean as surface current is in fact supplied by an undercurrent in the Indian Ocean rising to the surface farther west. And also the equatorial counter-current of the Indian Ocean must have some tendency towards counteracting the westward pressure among the islands. For those reasons the westward current on the north of Australia ought not to be given such importance on my theoretical chart.

At first sight it might be said that Mr. Halligan's chart of 1911, professing to give the actual course of the currents,

confirms my avowedly theoretical chart of 1866 as regards the flow from the Pacific to the Indian Ocean ; but in fact both charts are to the same extent mistaken as regards the importance of the westward current off the north coast of Australia. It has not the permanence of the current on the east coast or of that on the west coast.

You properly asked me to give authority for the currents I alluded to in this discussion : separated from their original context the remarks appear as the primary object of my contribution, instead of being merely incidental to the main argument. You do not appear to have asked Mr. Halligan to give authority for the currents depicted on his chart.

15th November, 1911.

I owe our ' Publication Committee ' an apology—rather a small one, but not too small to be, I hope, graciously accepted.

In my letter of 13th instant I suggested that they had not asked Mr. Halligan to give his authority for the ocean currents depicted on his chart. My excuse must be that the discussion has extended over eleven months.

This morning I suddenly remembered that in the Paper read last January he told us he had obtained them from the Admiralty Current Charts, making me resolve to look for those charts. I have to-day done so. There are more than a dozen of them, of various dates, from 1872 to 1897. Not one of them, standing alone, agrees as regards the Australian coasts with my chart of 1866, but the confirmation of that chart, published six years before any of the Admiralty charts, is very nicely given by Mr. Halligan having just recently extracted from them the same average course of the currents off the Australian coasts as given on my theoretical chart forty-five years ago.

Forty-five years of Admiralty researches are summed up by Mr. Halligan as giving precisely what was shown by the new theory of *the action of vis inertiæ in the ocean* before any of the Admiralty charts were published.

What more do our 'Publication Committee' want the new theory to do before they allow it to be heard in our Proceedings?

23rd November, 1911.

By excluding from the published report of the discussion on Mr. Halligan's paper my argument on the action of oceanic inertia our 'Publication Committee' have made themselves partisans of the current-creating action of the winds, of which Sir John Murray was until recently the most influential advocate. I say *until recently* because I think his researches in the North Atlantic with Dr. Hjort last year must have led him to modify his views as to the importance of wind action in the open ocean, without detriment to his investigations in enclosed or partially enclosed areas.

Be that, however, as it may, what I have here to deal with are Mr. Halligan's views.

Besides what I have already alluded to, there is another point on which Mr. Halligan's chart has the same misleading character as my chart of 1866. Both charts carry the current lines from the Coral Sea through Torres Strait and along the north coast of Australia into the central part of the Indian Ocean as if representing the same continuous stream of water. That is what I intended to show, as I was then giving the horizontal action of inertia too much importance in relation to the vertical action. The fact, however, is that the latter action brings an under-current from the south through the deep parts of the Indian Ocean and causes it to rise against the north coast of

Australia, filling the Arafura Sea with polar water, or a mixture in which that water predominates. Any of the equatorial water from the Coral Sea which may enter the Arafura Sea at Torres Strait must have a general tendency to go away north-westwards as a surface flow from the Australian coast towards the equator, exactly as is the case on the west coast of Africa and on the west coast of America in the same latitude.

It is true that there is a prevalent westward component in the set of the currents off the north coast, but not such as described on either of the charts. I supposed that the open passages from the Pacific, and the existence of land both north and south of the extreme east of the Indian Ocean would prevent the circulation from being similar to that of the corresponding positions in the Atlantic and in the Pacific; but that is not so, there is the same effective action of forces in all three localities.

I have already suggested that Sir John Murray's recent investigations must have modified his theoretical views; and as our Publication Committee have made themselves partisans of those views together with Mr. Halligan, I ask them to look at page 355 of Volume 37 of the Royal Geographical Society's Journal (187 F. in our library) and compare the section of temperature there given with the chart of the North Atlantic Currents which I published with my 'Treatise on the Action of Vis Inertiæ in the Ocean' in the year 1868, and which they can find reprinted facing page 129 of the second edition of 'The Ocean' (43 D. in our library), and also with Table I. at the end of my 'Essays on the Action of Astral Gravitation' (4 B. in our library). The column of temperatures in the Section given in the latter Table 700 miles east of Japan, which I constructed from the 'Challenger' records for the Pacific Ocean, exactly corresponds with the column of cold water in Dr. Hjort's

Section of the corresponding position in the Atlantic. The cold undercurrent shown in Dr. Hjort's Section is the counterpart of what exists in corresponding positions in each of the great oceans. It reaches the surface a short distance south of Dr. Hjort's Section and struggles against the warm flow coming from the equator, by which it is stopped on the surface of the ocean, but in the under strata it continues its course, to rise to the surface in the equatorial regions, chiefly in the east of the ocean.

By the current in the South Indian Ocean corresponding to that cold current shown on Dr. Hjort's North Atlantic Section the Arafura Sea north of Australia is filled with Antarctic water. By the action of those under-currents the ocean is at present lifted thirteen miles above its normal level over the equator; and with a gradually increasing velocity of the earth's rotation there would be a gradual increase in the strength of those currents, lifting the ocean more and more until at length it would rise away from contact with the earth's surface and revolve as a ring with a faster eastward velocity than the surface of the earth below it.

30th November, 1911.

A fountain raised by moderate and constant pressure forms an apparently stable hillock above the normal water level sustained by a constant circulation of its constituent particles.

When I first published, in the year 1866, the views of oceanic circulation dealt with in my letter of 23rd instant, it was generally supposed in the scientific world that the ocean is lifted round the equatorial regions by the earth's rotation into a position of stability which involved no persistent circulation of its particles.

The ' Treatise on the Action of Vis Inertiæ in the Ocean '

which I published in the year 1868 formed a systematic attack on that idea ; and in the year 1873 Dr. Todhunter published his ' History of the Theories of Attraction ' in which he reviewed the works of all the leading mathematicians who had dealt with the question of oceanic stability. The result he arrived at was to the effect that, though mathematical demonstrations had shown what conditions are requisite for oceanic stability, it had never been shown that stability must result from those conditions. (See *inter alia*, Vol. I., p. 196).

I urge upon you that Dr. Todhunter's work has removed all scientific basis for the manner in which my views are now being treated by our Publication Committee and other guardians of old school teachings. It is a mistake to suppose that there exists any mathematical demonstration at variance with them.

6th December, 1911.

In a letter dated 16th April, 1906, now published in Part II. of ' The Sling,' page 110, I recorded a protest against our Publication Committee making themselves partisans of theoretical views to such an extent as to disallow fair argument in opposition.

I propose now to point out how they stand in the history of scientific opinion as regards the theory of oceanic circulation which they are protecting from rival opinion in our published Proceedings.

Sixty years ago one of the great scientific events of the time was the discussion in which Sir John Herschell defended against the arguments published by Captain Maury the theory which treats the winds as the cause of oceanic circulation. Their published arguments are accessible to all students ; and I happen to be among those who consider that Maury dominated the subject with facts and arguments

which Herschell never faced. Be that, however, as it may, I ask our Publication Committee to look again at the under-current depicted on page 355, Volume 37, of the Geographical Journal alluded to in my letter of 23rd November. That is merely one of many similar currents forming a vast circulation unknown in that which Herschell declared to be just what would naturally result from the action of the winds. They are important facts discovered since Herschell formed his arguments, and they are part of the system of circulation indicated by the theory of oceanic inertia before there was any practical knowledge of their existence.

My paper was rejected in 1866 by the Royal Geographical Society on the supposition that the circulation it required did not exist. Now that what it required has been found to be the existing circulation, it is unworthy of science to twist the wind theory into rickety explanations rather than allow a fair hearing to the theory which required its existence before it had been discovered.

11th December, 1911.

I supplement my letter of 30th ultimo for a further attempt to make clear to our Council that they are bolstering up obsolete ideas to the detriment of the growth of scientific knowledge in our Institution.

I have in the above-mentioned letter and in other previous letters alluded to the works I published in the years 1866 and 1868 as having constituted a systematic attack on Galileo's idea of inertia on which Newton based his First Law of Motion.

In the paper on ' Steel Rails ' which I submitted to you under date of 21st December, 1900, I showed from the published works of the late Professor Hertz of Berlin that he had virtually emancipated himself from that so-called

First Law of Motion. And in a letter addressed to you on 19th March of last year, since published in Part V. of 'The Sling,' p. 320 (3 C. in our library), I attributed his acknowledged pre-eminence in scientific investigation to his having abandoned the wrong road in which the great majority of his fellow-workers were pursuing their investigations.

It seems to me that a very practical confirmation of my appreciation of Hertz's principles was given on 5th May of this year in the Royal Society of Arts in a lecture by one of his colleagues. In that lecture Professor Pictet of Berlin gave the simple definition of inertia as resistance to motion which has been a guiding principle throughout my published writings for forty-five years.

The current-creating action of oceanic inertia, dealt with in my contribution to the discussion of Mr. Halligan's paper, is a natural consequence of the definition of inertia given by Professor Pictet.

That contribution, dated 3rd February last, your 'Publication Committee' refused to allow to appear in our Proceedings; and just three months after that Professor Pictet came over from Berlin to teach for the first time in any London scientific institution precisely the doctrine of inertia for the admission of which I have been knocking at those doors for forty-five years.

Berlin is beginning to teach among London Professors what Berlin long ago got from a London amateur.

16th December, 1911.

The evidence accumulated in the past eleven years shows the 'due course' which our Publication Committee promised in your letters of 7th and 12th instants to be slow.

Whilst they have been keeping my Paper of 21st December 1900 on 'Steel Rails,' and that of 9th January

1907 on 'The Scrapping of the First Law of Motion' unnoticed, though the latter was written in response to a request from the Council, a Berlin Professor has come over to London, as recorded in my letter of 11th instant, for the express purpose of delivering a lecture in the Royal Society of Arts adopting, for the first time in any of our scientific institutions, the main point on which those Papers are based!

The fact is that our Publication Committee are blinding their eyes to new views which are surging round them and scrapping obsolete notions which they imagine the 'dignity' of their reserve to be sustaining.

I urge the Council to rouse their Publication Committee and save the Institution from becoming submerged in the scrap heap which has recently been piling up faster than ever before recorded in the history of science.

29th December, 1911.

Figure 1, Plate 5, Vol. CLXXXIV. of our Proceedings gives a chart of 'Ocean Currents of Australia' which has recently been constructed by Mr. Halligan from the Admiralty Current Charts to illustrate his Paper on the Harbours of New South Wales.

It is mere matter of fact that the coast currents on that chart all round Australia—that is to say the Great Southern Ocean current on the south, the current of the Coral Sea on the north-east coast, that of the Arafura Sea on the north coast, that of the Timor Sea on the north-west coast, that of the Pacific Ocean on the east coast, and that of the Indian Ocean on the west coast—are all identical with those depicted on a chart which I published in the year 1866 to illustrate the current-creating action of oceanic inertia, and reprinted in the New Principles of Natural Philosophy in 1885 (5 B. in our library).

All the Admiralty charts from which Mr. Halligan has constructed his chart have been published long after my chart of 1866: no such chart of Australian currents previously existed.

Secondly: To run in harness with the above confirmation of the theory of oceanic inertia as the chief cause of ocean currents I give the following confirmation of the same theory as regards its effect in controlling the course of free tide waves—that is to say, of waves which have ceased to be under the control of the luni-solar force by which they have been raised.

In the Article 'Tides' in the Ninth Edition of the 'Encyclopædia Britannica,' p. 373, Vol. 23 (24 G. in our library), Sir George Darwin reproduces Sir George Airy's chart of the tide waves in which the Atlantic tide wave on passing the north of Scotland flows southwards all across the North Sea between Scotland and the north of Denmark. But Map No. 23 of Berghaus's Atlas, Third Edition, published in 1892 (First Edition published 1886), that wave flows southwards only on the English coast, whilst on the coast of Denmark the wave flows northwards. And in the Eleventh Edition of the 'Encyclopædia Britannica' Sir George Darwin has substituted the Berghaus chart for Sir George Airy's, Vol. XXVI., page 943 (23 I. in our library).

It is mere matter of fact that in my chart already above mentioned I have given the *ocean current* a revolving motion in the North Sea—southwards on the coast of England and northwards on the continental coast: also, on page 116 of 'The Ocean,' Second Edition (43 D. in our library), I have said 'the tides, as they are broken by the coast lines, are constantly being thrown into the course of the ocean currents resulting from the action of vis inertiæ'; and the modification of Airy's tide wave in the North Sea which has been made in the Berghaus Atlas is therefore clearly a

confirmation of the action described throughout my works.'

Sir George Darwin, in the Third Edition of his work on 'The Tides,' page 198, comments on the difference between the charts of Berghaus and Airy in the North Sea without deciding as to their respective merits. I presume Berghaus has been guided by ascertained facts and not merely by my 'Treatise on the Action of Vis Inertiæ in the Ocean' or subsequent works.

Thirdly: In my letter of 11th instant I have recorded the fact of Professor Pictet of Berlin having, in a lecture delivered in the Royal Society of Arts last May, adopted the definition of inertia which is the basis of the tidal action which the Berghaus Atlas tends to confirm in the North Sea, and also of the current-creating action which Mr. Halligan's chart confirms as regards the coast currents of Australia. The Berghaus Atlas, in fact, gives the tide wave the same revolving action in the North Sea as I have given to the ocean current, thus making it, on becoming a free wave, follow the course of circulation caused by the current-creating action of oceanic inertia in accordance with *tendency* explained in my 'Treatise on the Action of Vis Inertiæ in the Ocean' and subsequent works—page 116, Second Edition of 'The Ocean' (43 D. in our library).

The position created by the foregoing may be summed up by saying that the basis of the theory which our Publication Committee have remorselessly rejected—preventing it from appearing in the discussion of Professor Vernon Harcourt's Paper on the Sandbanks of the Hooghly River, or in the discussion of Mr. Halligan's Paper on the Harbour Bars of New South Wales—has now been adopted in a

lecture in the Royal Society of Arts read in French by a German Professor.

The tidal action which our Council refused to allow to appear in the discussion of the Hooghly river sandbanks is certainly supported on a larger scale by the modification of Airy's tide waves of the North Sea which has been made in the Berghaus Atlas ; and as regards the current-creating action of oceanic inertia which they refused to allow to appear in the discussion of the harbour bars of Australian ports, it seems to me that though neither my chart of 1866 nor Mr. Halligan's chart of Australian currents is quite up-to-date, the identity of those currents as given in the recently published Vol. CLXXXIV. of our Proceedings with what are given on my chart published forty-five years ago makes the present position of our Publication Committee in the matter untenable.

I venture to hope that they will not consider it ' undignified ' to change their attitude towards the current-creating action of oceanic inertia to one of ordinary courtesy.

6th January, 1912.

Before passing to matters more directly connected with Mr. Halligan's paper, I must record a few more words on the tidal charts of the Berghaus Atlas, as the North Sea chart in that Atlas alluded to in my letter of 30th ultimo is not the only point on which Berghaus abandons Whewell and Airy in the direction indicated by the chart published with ' The New Principles of Natural Philosophy,' and also dealt with in my letter of 30th ultimo.

The tide waves were not marked on the First Edition of that chart in 1866, but were added on the Second Edition, published in 1868 : eighteen years before the publication of the First Edition of the Berghaus Atlas.

The Berghaus Map No. 20 gives a general chart of tide waves which is made almost as important by the blank spaces in mid ocean in which no waves are marked as for the information it gives in matters of detail.

Whewell's waves westwards from Australia to the South Atlantic and then northwards through the South Atlantic have been omitted. Those waves formed the most glaring point of difference between my chart of 1868 and Whewell's chart, as I give the waves eastwards all round the Great Southern Ocean, so that on rounding Cape Horn they have a northward as well as eastward motion. The Cape Horn wave as depicted by Berghaus is certainly a great step from the Whewell wave towards the adoption of that given on my chart of 1868.

And on the south of New Zealand Berghaus has *reversed* the direction of the Whewell waves and made them flow from west to east as depicted on my chart.

Those waves glaringly sweep away the original basis on which the theory of the controlling action of the Great Southern Ocean tide wave was founded. The wave was supposed to be orginated by luni-solar action in the South Pacific and then to follow the moon westwards until blocked by the coast of South America and turned northwards as a free wave.

I have brought the Berghaus Atlas before you because for the elucidation of the action of oceanic inertia (as stated in my letter of 29th December) its charts of tide waves serve well to run in harness with the chart of ' Ocean Currents of Australia ' which you have published with Vol. CLXXXIV. of our Proceedings.

I beg the Council to look at the Berghaus Tide Charts and then at their letter of 4th July, 1905, together with the comments on it given in my letters of 4th and 8th June, 1908, published on pages 111 and 112, Part II., of ' The Sling ' (3 C.

in our library). It is evident that what I said on 17th January, 1905, in the discussion of Professor Vernon-Harcourt's Paper on the Sandbanks of the Hooghly River was suppressed in the published report of our Proceedings under the supposition of its being beneath the dignity of the Institution to discuss the idea of the course of the tide waves of the Great Southern Ocean being eastwards against the moon's motion all round the earth. The Berghaus Chart clearly rejects the idea of those waves following the moon as supposed by Whewell. But even if I had continued to stand quite alone in reversing the direction given to the tide waves, not only where Berghaus does so, but also all through the Pacific, the Indian, and the Atlantic portions of the Southern Ocean, that would not, in face of my published arguments on the subject, have justified such an act of discourtesy as the obliteration of my spoken words from the official report of our Proceedings.

I hope I have now submitted evidence enough to lead the Council to recognise and repair the mistaken course they took in connection with the discussion of the Hooghly River sandbanks on 17th January, 1905.

15th January, 1912.

I duly received your letter of 9th instant.

I do not know who 'the Committee' you write for are, but I do know that the Council, whose names I have before me, are responsible for the conduct of the Institution, and feel sure that they would not knowingly sacrifice its character for the gratification of any pique. If I thought it possible for that not to be a true appreciation of the position, I should refrain from frankly pointing out that it is not possible for us to prevent 'the matters to which you allude' from being 'carried further.' The truth in such matters will

not allow itself to be long smothered. And as regards the 'reopening' which you tell me your Committee refuse to allow, the fact is that the subject of Professor Vernon-Harcourt's paper on the Sandbanks of the Hooghly River was unconsciously 'reopened' by themselves in allowing the publication of my chart of Australian coast currents in our Proceedings without acknowledgment, on the mistaken idea of those currents having been discovered by practical researches by the Admiralty. Due acknowledgment would have been conscious reopening.

I have abundantly shown in the letters you mention that the Council have printed in Vol. CLXXXIV. of our Proceedings my chart of the course of the coast currents on all sides of Australia. The currents on the north-west, the north, and the north-east coasts are not in the aggregate obtained by practical researches, for the simple reason that no such continuous current from the Coral Sea through the Arafura Sea and the Timor Sea into the South Indian Ocean has ever existed to be discovered by any explorers. My 1866 chart was a preliminary attempt to locate in the ocean the circulation indicated by the theoretical action of oceanic inertia, and your chart has evidently in some indirect manner adopted it so far as the currents off the Australian coasts are concerned.

In the first edition of 'The Ocean,' published in 1873, I gave merely a skeleton diagram of the main currents all over the world without attempting to locate them on a true chart; and in the second edition, published in 1885, I again located them on a chart on the supposition of the ratio of the vertical to the horizontal action of inertia being much greater than that by which I was guided when drawing the chart which I published in 1866, from which the Australian coast currents in Vol. CLXXXIV. of our Proceedings have been copied. Roughly speaking, all the

main currents are correctly located on that 1885 chart, but the ratio of vertical to horizontal action requires to be still further increased to make some matters of detail correct.

Even if the appropriation of my work in Vol. CLXXXIV. of our Proceedings stood alone I could not reasonably be expected to refrain from comment. But, in fact, the antecedents of your Committee's action in connection with the subject made some comment imperative. They first rudely struck out from the official report of our Proceedings my contribution on the subject of tidal action in the Hooghly River, and then, on results of my work appropriated on the chart of Australian coast currents coming before them, they publish the appropriation of my work and at the same time strike out from the official report of our Proceedings my explanation of the circulation, in which I carefully avoided the wrangling they now force upon me by their partisan action.

Until Mr. Halligan's replies in discussion had been received from Australia and published in Vol. CLXXXIV. of our Proceedings I could not tell in what position the subject would be placed.

The reading of Mr. Halligan's paper gave a suitable opportunity for discussing the action of ocean currents on the sand movements at harbour bars. In face, however, of the manner in which my spoken words on Professor Vernon-Harcourt's paper on the Sandbanks of the Hooghly River had been obliterated from the official report of the Proceedings I decided not to enter into a verbal discussion, but limited myself to suggesting that general principles are applicable to a greater extent than supposed by the writers of the papers which had been read, and said I should be glad to contribute an argument in writing to that effect if the President could assure me that it would receive consideration.

That assurance was given by the President.

Notwithstanding that assurance, my argument on the subject was excluded from the official report! Your 'Committee' preferred to break the assurance given by our President rather than allow my views to appear in the discussion.

The excluded argument is that by which I constructed the chart from which the Australian coast currents which you *have published* have been in some indirect manner copied!

It seems to me absurd for your Committee to imagine it possible for me to acquiesce in the idea of the subject being closed: it will from time to time reopen itself in spite of any precautions your Committee may take, and would do so even if I were tamely to allow myself to be effaced—which I shall not do.

22nd January, 1912.

The three Papers read on 24th January of last year on Australian Harbours, together with the discussion extending over eleven or twelve months, form a dissertation on the action of permanent ocean currents and tidal currents on the harbour bars as well as on the action of the winds in creating ocean currents.

The official report of the 'Challenger' Expedition treated the warm current which flows southwards off the coast of New South Wales as a natural consequence of the prevalent winds; and your Publication Committee have protected the 'Challenger' theory from opposition by refusing to record in our Proceedings my argument to the effect that it is part of a general circulation which results from the inertia of the water causing the ocean to resist the effort of the earth's rotation to carry it eastwards.

My few strictures on the idea of the main currents of the ocean being caused by the winds, which you have

published, were given as merely supplemental to the argument to the effect that those main currents are a consequence of the earth's rotation. Without the latter argument they constitute an attack on a widely accepted theory without offering anything better to put in its place. The 'Challenger' imagined herself to hold a brief for the protection of 'orthodox' science against my demonstrations to the effect that inertia is simply the opposite aspect of universal gravitation, according to which the ocean is carried westwards in bulk in the equatorial regions by the drag of astral gravitation against the earth's rotation, whilst the reaction of the earth's gravitation at the same time carries an equivalent mass of water eastwards in the two temperate zones. I know quite well that there is a wind-drift superimposed on that circulation and that in some places it has important effect. The main features of the circulation are, however, quite independent of the winds. Where the mass of equatorial water reaches the coast of Brazil it is not the dark blue water that has crossed the Atlantic as surface water that is thrust against the shore line; but, in fact, the under-strata, as the ocean shallows, are thrust to the surface and are more or less green in colour. The dark blue water flows southwards in a broad and deep stream across and against the strongest parts of the South-East Trade Wind, whilst the water from the under-strata packed against the coast is comparatively free to be blown wherever the wind may attempt to drive it. That all shows the winds to play a very subordinate part as current-creating forces.

In the same manner as the coast of Brazil, in stopping the westward motion of the Atlantic, brings water from the under-strata to the surface along the coast, so also the north-east coast of Australia, together with the north-east coast of New Guinea, stops the westward motion of the Pacific and forces water from the under-strata to the surface

along those shores. In that manner the blue surface water of the equatorial regions is turned southwards off the east coast of Australia. And I think it probable that another stream of that surface water passes from the north part of Torres Strait north-westwards into the Banda Sea. It certainly does not flow *westwards* through the Arafura Sea and the Timor Sea, as I supposed when I drew the chart which has been partially copied in Vol. CLXXXIV. of our Proceedings. Those seas are supplied by an under-current in the Indian Ocean which rises to the surface along the north-west coast of Australia, and then tends to drift north-westwards towards the equator.

The converging trade winds tend to make the water *bank down* against the north-east coast of Brazil; but, instead of such effect being found in the equatorial regions, the fact is that the under-strata are *lifted* there by the action of the earth's rotation and throw the surface water off in streams running across and against the trade winds on each side of the equator.

I urge on the Council that the manner in which their ' Publication Committee ' are protecting the obsolete notions which the ' Challenger ' fancied herself called on to support is injurious to the best interests of our Institution.

12th February, 1912.

One of the leading features of Twentieth Century Science, so far as the Man in the Street is concerned, is the public appearance of the Great Western Railway Company as a teacher on the subject of oceanic circulation. Their chart of Atlantic Ocean Currents has been prominent in our railway stations for some time, but it was only a few days ago that, as it chanced to be near me when waiting for a train, the idea suddenly struck me that the influence of

the Great Western Company may have played some part in preventing my few words in the discussions on the sand-banks of the Hooghly River, the Netravati River, and Australian Harbour Bars from being fairly recorded in our published Proceedings, and I therefore submit the following points on which the Great Western chart is erroneous, though for each such point the Company may cite the authority either of Benjamin Franklin and Captain Maury, or that of James Croll. The chart follows them on important points on which their teachings have become obsolete. The actual course of the circulation is not what they inferred from the knowledge they possessed.

There can, I think, be little doubt that warm water from the Gulf of Guinea reaches the coast of Cornwall; but it does not go near the Gulf of Mexico in its course as supposed by the Great Western Company.

Half-way across the Atlantic along the equator a vast mass of the westward current of the equatorial regions branches off north-westwards, turning northwards and eastwards to the shores of Europe. It meets the Gulf Stream, flowing from the Gulf of Mexico, off the Banks of Newfoundland; but the water of the latter stream is then already sinking downwards to continue its course as under-currents to Baffin's Bay and Iceland, and probably also as an under-current towards Cape Verde. The Gulf Stream is a rivulet compared with the great mass of water which flows northwards through the central part of the North Atlantic, and it is the water of the latter stream that warms the eastern part of the ocean from Spain to Spitzbergen. The idea of there being such a central mass of still water in the North Atlantic as depicted on the Great Western Company's chart is an obsolete delusion. The main North Atlantic current flows right through the region which the chart supposes to be still water.

Another obsolete delusion depicted on that chart is the idea that a great surface stream is driven by the South-East Trade Wind across the equator into the North Atlantic and then into the Gulf of Mexico. That delusion originates in the fact that vessels meeting the South-East Trade Wind north of the equator find themselves in colder water than farther north, and have assumed that the preponderating strength of that wind brings the cold water from the far south. The fact, however, is that the cold water rises from the bottom of the ocean in that and other parts of the great westward current of the equatorial regions. Immediately south of that region of cold water the South-East Trade Wind blows across a *warm* stream of the equatorial current without affecting its course; so that, if the wind carried much of the surface water along with it, that surface water would become still more warm as it reached the equator in the splendid sunshine which prevails in that part of the ocean. The fact is that a great part of the water which flows westwards from Cape San Roque turns back eastwards in the Atlantic counter-current; and the Gulf of Mexico is mainly supplied by under-currents which are the oceanic counterpart of the North-East Trade Wind and rise in the south and west parts of the gulf and in the Carribean Sea, where the prevalent surface drift is north-westwards across that sea. The trade wind tends to drive surface water from the Carribean Sea into the Gulf of Mexico; but the normal north-west current of that sea would to some extent pass through the gulf without the assistance of the wind.

It is deplorable that whilst the Great Western Railway Company are placarding London with obsolete ideas as to the course of oceanic circulation, the Council of our

Institution are at the same time remorselessly doctoring our official records for the purpose of preventing the explanation given by the action of oceanic inertia from being recorded in the discussions of the subject in our Institution.

In each of the three discussions which I have above mentioned, our Publication Committee with their shorthand notes of my words before them seem to have supposed themselves to hold the position of masters with a schoolboy's essay before them for revision instead of having the simple duty of seeing that reasonable arguments of contributors to the discussions are fairly recorded. It is not in the interest of the Institution that they should arrogate to themselves the position of censors responsible to prevent the appearance of any heretical scientific views in the publication of our Proceedings. The solution of the question of oceanic circulation through the inertia of the water resisting the earth's rotation—or, in other words, by the conflicting action of astral and terrestrial gravitation—marks an epoch in the history of science and is in fact the unacknowledged basis of much of the progress of scientific knowledge that has been achieved since it was published more than forty-five years ago.

10th April, 1912.

In the Lecture Hall of the Royal Institution on the Friday evenings of 26th January and 2nd February the Institution of Civil Engineers made a bold show of supporting Newton's First Law of Motion. My present object is to give a frank and open reply to those lectures, as they clearly constitute a combined and deliberate, though unavowed, attack on my work. I must, however, begin by urging, as I have done I do not know how often before, that my work is not destructive, but constructive. It shows how the gravitation of the stars supports the spinning top against

the earth's gravitation, and it permeates engineering science. It is only because that law was used to thwart my work that I found it essential to make it an object of open attack.[1]

In the first of the two lectures Professor Bertram Hopkinson of Cambridge began by telling us that it was so long since many of us had left school that we perhaps might not remember accurately Newton's laws of motion; and that, as his arguments throughout the lecture would be based on them, he thought it desirable to commence by giving them to us in a short explanatory statement.

In view of the manner in which those laws have been treated in the Cambridge edition of the 'Encyclopædia Britannica' and criticisms I have addressed to the Royal Institution regarding that treatment, I was perhaps more eager than anyone else present to hear the Cambridge Professor's explanatory statement of them, but I was quickly disappointed by his plunging into the theory of projectiles without any shadow of the promised statement; and, in fact, the most important point of Newton's first law of motion which I attacked and scrapped some years before Professor Hopkinson was born was completely ignored by him. It was not involved in any of the explanations he gave. As far as that first law is concerned he was guided, not by Newton's law, but by the corrected form given to it in the works I published before the young Cambridge Professor was born. It seemed to me at times that the lecturer was better acquainted with my works than with Newton's. Be that, however, as it may, in the latter part of the lecture he apologised to the Royal Institution, with Lord Rayleigh in the Chair, for venturing before so distinguished an audience on a new explanation of the action of the cap which is now always placed over the point of heavy projectiles. Knowing

[1] Pages 401 to 405 are virtually a repetition of the letters of 28th February and 31st March to the Royal Institution.

what I know in connection with that subject, it was not possible for me to be otherwise than on the *qui vive* to hear the explanation which I gave on the 10th February, 1903, in the Institution of Civil Engineers expounded to the Royal Institution, as the repeated apologies for what was about to be said seemed to preclude the generally-accepted explanation, and I can imagine no alternative but that which I gave in the discussion in the Institution of Civil Engineers as recorded on pages 47 to 49 of Vol. CLIII. of the Proceedings, But, notwithstanding the flourishes of trumpet with which it was heralded, I could not see that the explanation given differs intrinsically from what I have always understood to have been generally received. It was impossible for Lord Rayleigh to object to the application of the simple term *inertia* to the action of the cap in that explanation. As the lecturer had refrained from using the term in the sense of momentum no confusion could be created by its use in the sense of resistance to momentum. But, using the terms in the same sense as Professor Hopkinson, I say the momentum, not the inertia of the cap, makes the plate vibrate and so weakens its resistance before the main blow of the shell reaches it. Unless the lecturer was playing fast and loose with the meaning of terms, what he gave us was the old explanation and not a new one.

I now pass to a more important part of the lecture. Perhaps Professor Hopkinson is right in suggesting that the long time elapsed since I left school may have resulted in my forgetting Newton's treatment of some subjects. It certainly does seem to me strange that Professor Hopkinson should apply Newton's methods to *relative* instead of to absolute motions of projectiles. It is true that Newton dealt only with relative motion in the heavens. But that was simply because he never succeeded in finding a basis for determining absolute motion beyond the range of terrestrial

phenomena. His analysis of the forces exerted by terrestrial bodies in motion was, however, based on absolute motions in practical experiments ; and his elaborate dissertation on the manner of distinguishing between absolute and merely relative motion seems to me to show one of his great ambitions to have been to find a basis for determining in the heavens absolute velocities of motion as defined by him. His misunderstanding of the ratio of forces involved in the action of the sling made it impossible for him to distinguish absolute motion in the heavens ; but, after discovering the true ratio of those forces I was able to show, in a letter I addressed to the Royal Institution on 15th May of last year, that Rankine's law of potential energy combines with Newton's law of gravitation in the motions of the planets exactly as it does in the motions of projectiles, so giving the velocities of absolute motions of the moon and planets in the sense in which Newton defined it in terrestrial phenomena. The figures I have given in the above-mentioned letter make it clear that both in astronomy and in terrestrial physics absolute velocity is as the square root of motive force and the resisting action of inertia as the square of velocity under similar conditions. In the heavens that is shown by Newton's law of gravitation combined with Rankine's law of potential energy to be the basis of the action of physical forces as already above stated ; and in terrestrial phenomena it is confirmed by Newton's numerous experiments with pendulums. Some of the arguments and figures given by Professor Hopkinson seem irreconcilable with that basis. For instance, on page 13 of the recently printed issue of his Paper he treats the pressure of two bodies meeting at a relative velocity of 2,000 feet per second as a fixed quantity not affected by what their respective absolute velocities may be. But, in fact, if one body be at rest and the other moving with the absolute velocity of 2,000 feet per

second the pressure is represented by $2{,}000^2 = 4{,}000{,}000$; whereas if the two bodies meet, each with a velocity of 1,000 feet, the pressure is then represented by $1{,}000^2 + 1{,}000^2 = 2{,}000{,}000$. That is to say—if the relative velocity of 2,000 feet is due to equal absolute velocities in the two bodies the relative pressure is only half what would be created by the absolute velocity being all in one of the bodies. It is dangerous in physics to base arguments on anything but absolute quantities, whether of matter or of motion.

In the second of the two lectures Professor Elphinstone commenced with having the Lecture Hall darkened, and a lantern slide threw on the screen in large letters Newton's First Law of Motion, to be carefully read and re-read by the audience as giving the key to the action of the Gyroscopic Compass which was the subject of the lecture.

It was evident that the dramatic performance was expected to obliterate any opponent of the great law which glared from the screen.

Throughout about three quarters of an hour experiments were shown in which phenomena were plausibly explained by that law tending to prevent the gyroscope changing from the plane in which it had been set rotating. In each of the experiments there was, however, one direction for change of plane in which such change was prevented by the mechanism of the gyroscope or of its supports, so that, if it were granted that Newton's law gave the true explanation for the plane not changing in the direction in which it was not mechanically restrained the question arose as to whether the law would avail to prevent change of the plane if not mechanically prevented in the other direction.

A large gyroscope which had been hanging by a long cord from the ceiling was at length approached by the lecturer, who was evidently conscious of it being the *pièce de résistance* of

the evening. It was the first experiment in which the gyroscope was free to change its plane in all directions.

The scene we then had before us so nearly approaches the incredible that I dread describing it at the risk of being considered a romancer. The gyroscope was rotating in a north and south plane. The lecturer attached to one side of it a weight in the manner which had failed to cause change of plane in previous experiments. He then triumphantly called on us to witness to the manner in which Newton's law was vindicated by the gyroscope *refusing to change its plane of rotation*.

The glaring fact, however, was that the instant the weight was attached to the gyroscope it began to change its plane of rotation, and in less than a minute it was rotating in an east and west plane instead of its previous north and south plane—the lecturer all the time in triumphant tones calling on us to notice how it vindicated Newton's law by *not* changing its plane.

The reason why in a preceding experiment, with a model of the Brennan monorail car, a similar action did not cause a change of plane was simply because the pressure of the flanges of the wheels against the rail prevented the car from turning round so as to change the gyroscopic plane of rotation ; and the evidence given by the hanging gyroscope to the effect that the mechanical action of the rail—not Newton's law—keeps the car from turning round, makes it absurd to suppose that Newton's law gives the true explanation of the monorail car not capsizing.

The hanging gyroscope experiment was applauded at the meeting through a confusion of ideas. Though the audience had just seen that the weight did not capsize the monorail car they were evidently watching for a capsizing movement of the hanging gyroscope instead of the turning motion which the rail prevented the Brennan car from making.

Sir William Crookes was evidently immensely pleased with the manner in which the two Members of the Institution of Civil Engineers supported him against my works, but his applause of the hanging gyroscope experiment was far from being unanimously followed.

The key to the explanation of gyroscopic action was first given on 3rd November, 1877, in a lecture which I delivered in Willis's Rooms, and afterwards extended in my 'Essays on the Action of Astral Gravitation in Natural Phenomena.' The position of the controlling stress was unknown until I gave it in the above-mentioned lecture ; nor, so far as I know, had it ever before been suggested that the gyroscope is controlled by the reaction of vis inertiæ against its motion—not by the direct action of the motion. Those views cannot logically be admitted without discarding Newton's definition of vis inertiæ and substituting for it a recognition of the simple fact that inertia is merely the opposite aspect of universal gravitation, as shown forty-four years ago in my 'Treatise on the Action of Vis Inertiæ in the Ocean,' and fifty-eight years ago in verbal argument.

I am, dear Sir,

Yours truly,

Wm. Leighton Jordan.

APPENDIX I

THE STANDARD OF VALUE

To the Right Honourable Herbert Henry Asquith, K.C., M.P., His Majesty's First Lord of the Treasury

ROYAL SOCIETIES CLUB,
28th August, 1912.

Sir,—Whilst we were in the midst of the turmoil of the Railway Strike last year I estimated from the Stock and Share lists in the 'Economist' that the aggregate annual amount of preferred interest which the railway shareholders have to pay before any dividend becomes available on the Ordinary Shares is more than £15,000,000.

As I have for more than forty years held with decision views of some old economists whose teaching has been set at defiance in currency legislation from the year 1870 to this day, and as one of the most eminent of the currency legislators during that time, Sir David Barbour, has just published a work on the Standard of Value advocating views which, when Sir Isaac Newton was Master of the Mint, were supposed to have been finally rejected by all statesmen as pernicious to the welfare of the world, I venture to offer to His Majesty's Government an appreciation of the present position of the world's metallic currency.

Mr. Sauerbeck's Index Numbers show the purchasing power of the above-mentioned £15,000,000 to have been twenty per cent. greater than it would have been if prices

had remained in accordance with the average of the ten years 1868 to 1877. That is to say, the appreciation of the pound sterling which had occurred in the meantime was causing the equivalent of £3,000,000 of the earnings of the railways to go to money-lenders instead of to the shareholders, who were thus squeezed between the holders of preferred capital and the wage-earners of the railways. A continuation of the prices which prevailed in 1868 to 1877 would have given the shareholders the equivalent of £3,000,000 larger share of the profit accruing in the working of the lines, which would have enabled them easily to satisfy more than the demands urged by wage-earners for higher pay and remain themselves better off than they now are.

The lull which now prevails in the strife between railway shareholders and their wage-earners makes this an opportune moment for looking dispassionately at the position of the question. Why has our Standard of Value been so disorganised as to have created difficulty between employers and employed ?

For some time before the battle of Waterloo our silver and gold coins were at a premium, in relation to the paper currency, estimated by some writers as having been at one time as great as 100 per cent., and the country had become almost destitute of metallic money.

On the restoration of peace, Parliament ordered the Mint to coin gold sovereigns for all comers as legal tender for the pound sterling, but to keep the coinage of silver for the public in abeyance pending an order which might at any time be given by the King in Council without further reference to Parliament. That arrangement was a practical recognition of the fact that a metallic currency could be most quickly supplied by coining gold, whilst at the same time recognising the validity of the arguments by which John Locke and

others had indicated the disastrous effects which would result from the exclusion of silver from its place in the Standard of Value. The action of Parliament at that time constituted a formal repudiation of Lord Liverpool's arguments for the exclusion of silver from our Standard of Value.

Up to the time of the Franco-German war the Paris Mint maintained the stability of the Double Standard of gold and silver throughout the civilised world ; and as soon as the action of the Paris Mint became doubtful, compliance with our Parliamentary legislation of 1816 required an Order in Council to be given for our Mint to coin silver as well as gold for the public as full legal tender. But instead of that course—deliberately prearranged as requisite for the national welfare—the advocates of Lord Liverpool's views induced Mr. Robert Lowe as Chancellor of the Exchequer, with Mr. Gladstone as Prime Minister, to pass an Act in the year 1870 which permanently closed the Mint against the coinage of silver for the public, thus giving this country a gold standard by law.

This was followed next year by Germany, and three years afterwards by the United States, also adopting the gold standard, at the same time increasing the disturbance of values by forcing sales of silver to facilitate the circulation of gold. And in the following year, 1874, France closed the Paris Mint against coinage of silver for the public and also became a gold standard country.

To those economists who recognise that the values of gold and silver are determined by the simple laws of supply and demand, it cannot seem strange that the above legislation for increasing the demand for gold and for, at the same time, restricting a demand for silver, was followed by a fall in the exchange value of the silver currency of India in relation to the gold currency of this country. The idea of that

fall of the silver rupee in gold exchanges being a direct consequence of the legislation above referred to is not admitted by all who have been chiefly responsible for the legislative Acts which have created the present position of the world's metallic currency; but theory on that point may be considered as quite beside the main questions which are now at issue. It has become clear that the changing relative value of gold and silver was for many years due to a rise in the value of gold without any important fall in that of silver. Silver, however, at length began to fall in value so rapidly that in the year 1893 the Government of India found it necessary to restrict its coinage in order to prevent what threatened to become a disastrous rise in prices throughout the country; and that was followed in the year 1899 by an Act making the gold sovereign legal tender as the equivalent of 15 silver rupees. During the subsequent twelve years India has absorbed £125,000,000 in gold at a roughly increasing rate; £50,000,000 having been taken during the last three years.

Is it wise—I will not say merely in the interests of commerce, but rather in the interests of civilised government—to force such a substitution of gold for silver? China and others are expected to follow the lead given by India and Japan, and in such case the demand for Asia may easily become £100,000,000 a year for a long time. Where is the gold to supply £1,000,000,000 to Asia in ten years of a not distant future without prospect of creating disastrous scarcity in Europe and America? The risk we have before us is that such difficulties between employers and employed as that of which I have given a small example above may make organised government impossible in such gold standard countries as have vast credit systems current between debtors and creditors.

The Government of India, by opening a sluice to turn the

gold supply of the world into Asia, have created a position which makes it imperative for this country to return to the Double Standard of Value which was discarded by the legislation of Mr. Gladstone and Mr. Lowe in the year 1870. I say *imperative* because the alternative is the prospect of a sequence of more or less turbulent wranglings between employers and employed, compelling sooner or later resort to a forced paper currency to carry on the government of the country.

Sir David Barbour, who is pre-eminently responsible for the present position of the world's metallic currency, has well chosen this as an opportune moment for issuing a new work on 'The Standard of Value.' Though for many years a professed bimetallist, his chapter on 'The Cause of the Divergence in the Relative Value of Gold and Silver between 1873 and 1886' is of itself sufficient to show that he has never fully grasped the theory of the Double Standard. Throughout that chapter he ignores the *now* generally admitted fact that the closing of the European and United States Mints against silver in the years 1870 to 1874 was *the* cause of the relative depreciation of silver; and he treats the fall in the Indian rupee exchange as caused by the fall in the gold prices of commodities exported from gold standard countries to India: that is to say, he treats as cause and effect what all genuine bimetallists recognise to be two inseparably concomitant effects of the same cause; and that error pervades the work in really superfluous arguments which unnecessarily complicate and mystify the points under discussion.

In the chapter on 'The Abandonment of Bimetallism' he gives such prominence to Germany having taken steps in December 1871 for changing from a silver to a gold standard as to make him appear unconscious of the fact of the gold standard having been legally established in this

country only the preceding year by Mr. Gladstone's government. It cannot be supposed that either Germany or the United States would have incurred the expense and trouble of changing to a gold standard if England had not taken the lead in that direction in the year 1870 If historical antecedents are to be used to influence any suggested course of action they ought to be correctly given. Sir David Barbour's statement that 'the world drifted into bimetallism and drifted out of bimetallism without foreseeing the probable consequences of either change' is also glaringly at variance with the history of the subject. When Sir Isaac Newton was Master of the Mint the writings of his intimate friend, John Locke, were in the hands of every statesman in this country, explicitly describing the mischievous consequences which would certainly result if silver were discarded from the standard of value. And in the year 1873 Benjamin Disraeli made his memorable statement—'I attribute the great monetary disturbance that has occurred, and is now to a certain degree acting very injuriously to trade—I attribute it to the great changes which the Governments of Europe are making with reference to their standards of value.' The fact is that the financiers who have succeeded in separating the gold from the silver standard as advocated by Lord Liverpool have done so in defiance of persistent demonstration in argument of the mischievous consequences of such a course.

The current number of the 'Quarterly Review' shows a true appreciation of the importance of the question involved in the battle of the Standards where it says the tyranny of finance is a more actual danger than Mob Law and Labour tyranny—it is at once more ubiquitous, more insidious, and difficult to shake off or to control; and in those words the 'Quarterly Review' gives a clear echo from the writings of supporters of the double standard thirty

years ago, pointing out the ruinous consequences which persistence in legislating under the guidance of Lord Liverpool's arguments would certainly entail.

I have the honour to be, Sir,

Your most obedient, humble servant,

WM. LEIGHTON JORDAN.

APPENDIX II

STABILITY OF AEROPLANES

To the Editor of the 'Times.'

ROYAL SOCIETIES CLUB,
24*th September*, 1912.

Sir,—In your issue of 12th instant you commented on the two accidents with Army Aeroplanes which had just recently occurred, and called the attention of your readers to a letter from an engineering correspondent, Mr. Cloud, as offering a technical explanation.

In that letter Mr. Cloud says that the dynamics of the engine and the propeller are well understood, and that the magnitude of the forces is proportional to the product of four factors, viz.—

m the mass rotating.

r^2 the square of the radius of gyration.

ω the angular velocity of such rotation; and

a the angular velocity of change in the orientation of the axis of rotation.

The general tenor of your article seems to be to the effect that in connection with the action of forces involved in our flying machines we have still something to learn; and that view seems to me to be clearly justified by Mr. Cloud's mathematical formula, which is certainly erroneous on more than one point.

In a letter I addressed to you on 14th instant, which you are declining to publish, I pointed out :—

1. That a radical error in that formula is that it squares radius instead of velocity, so that if correct for any given velocity it must of necessity be wrong for any other velocity of rotation. If it has been found sufficiently correct for practical purposes in any engines of slow rotation, its error must increase by leaps and bounds as faster velocities have to be dealt with.

2. Besides that radical error, it is also a mistake to deal with the angular velocity of rotation. The velocity which ought to be squared instead of the radius is the absolute velocity of the motion as defined by Newton.

3. In most mechanisms it may be safe to square the velocity of the radius of gyration, but in others the greater velocities beyond the radius of gyration may be of practical importance, seeing that the local stresses increase as the square of the velocity of each part.

4. This latter point becomes of supreme importance in connection with any ' change in the orientation of the axis of rotation.'

Mr. Cloud gives no demonstration of his formula in figures which would enable criticism to be more practical ; but a point in his letter may be amplified to facilitate a more definite discussion.

Mr. Cloud says the deflecting forces cause the forward part of the aeroplane to tip downward in the case of turning in one direction, and to tip upward in the case of turning in the opposite direction.

The fact is that if in the rotation of the engine and propeller the motion of the upper part is from port to starboard, then by turning the bow of the aeroplane to starboard the combined velocity of starboard motion is increased and that of port motion diminished, so that, as vis inertiæ

resists the motion as the square of its velocity, there is a predominant clutch of resistance against the motion of the top of the rotating mechanism which tends to force it downward.

If the turn is made to port, instead of to starboard as above, then the predominant clutch is against the motion of the bottom part of the mechanism and tends to force it upward.

By similar action, if the bow of that aeroplane is turned downward, then the predominant clutch of resistance is on the starboard side, which is rotating downwards, and that action tends to force the mechanism to port.

If the bow is turned upward, instead of downward as above, then the predominant clutch of resistance is on the port side, which is rotating upward, and its tendency is to force the mechanism to starboard.

I have in my published works shown that the swerving of a golf ball, when struck so as to give it a spinning motion in its course, is explained by precisely the same action as above described ; and I have also specified practical experiments which show all other hitherto accepted explanations to be obsolete.

The main point to be borne in mind in the theory of the subject is that the directly controlling force in all gyroscopic action is not the motion, but the vis inertiæ resistance, which is as the square of the velocity which it resists and is merely the opposite aspect of universal gravitation.

I am, Sir,

Your obedient servant,

Wm. Leighton Jordan.

APPENDIX III

OCEANIC CIRCULATION

The Institution of Civil Engineers' printed version of a written contribution dated 3rd February, 1911, all the main points of which were excluded from publication in the Proceedings. See letters addressed to The Institution of Civil Engineers, pages 377 to 397.

Mr. Wm. Leighton Jordan thought everybody would agree that the sand was raised by wave action, and that being raised it went away with the current as Mr. Halligan had observed on p. 14. He wished to show why there was a great difference in the manner in which sand was carried on the east and west coasts of Australia, and to indicate that where the action of coastal currents had to be dealt with in engineering works, general principles were available for guidance to a greater extent than had been recognised in any of the three Papers. The southern half of Australia was washed on its western side by the anti-cyclonic current of the Indian Ocean, and on its eastern side by that of the Pacific Ocean. As the Indian Ocean current flowed towards the equator it entered latitudes of increasing velocity of motion in the earth's rotation, and therefore tended to fall away westwards and to cause cyclonic eddies to be formed north of any point at which it touched the coast. As the Pacific Ocean current, on the other hand, progressed toward the Pole, it entered latitudes with decreasing eastward velocity, and its acquired

eastward momentum, therefore, must give it also a tendency to trend away from the coast and to cause cyclonic eddies to be formed south of any point at which it touched. Such an eddy would seem naturally to account for the sand-dunes to the south of the North Head at the mouth of the Richmond River on the east coast. And on the west coast the action of such eddies seemed to be apparent in the formation of sandbanks between the ocean current and all the coast line north of Cape Leeuwin, where the current touched the shore. There existed, however, an important difference in the action of the anti-cyclones on the east and west coasts respectively. The inertia of water, resisting the effort by which the earth's rotation carried it eastwards, was more effective in the surface water than in the lower strata, because the latter were more subject to frictional resistance consequent on their being more firmly held down by the earth's gravitation; they were therefore more easily carried round with its rotation. For that reason the predominant thrust of the water against the Pacific coast was given by the surface water, and against the Indian Ocean coast by the bottom water. There was another important difference in the action of ocean currents on the east and west coasts respectively. On the west coast the current from the south-west dominated that part of the ocean. Its bottom stratum brushed the sand against the coast, and its own inshore eddies tended to keep the sand revolving with them. But on the east coast there was an independent undercurrent from the south bearing against some parts of that coast, as there also was in the same latitude against the east coasts of South America and South Africa. Part of that undercurrent rose to the surface in the Tasman Sea, and ran northwards as a counter-current just outside the warm coast current which ran southward. Those independent currents did not tend to create such a series of revolving eddies as existed on the

west coast, because wherever they met one was borne back by the other until a position of equilibrium had been established between them, when each smoothly took its own course. As the westward drag of inertia varied as the square of the velocity of the earth's rotation, it became greater in the equatorial regions than in high latitudes. If it were supposed that in the equatorial half of the ocean a normal tendency existed to cause a lagging at the rate of 4 miles an hour, and in the polar half at 2 miles an hour, then in relation to the earth's surface the dragging force was represented by $4^2 + 2^2 = 20$. But by the greater force of the equatorial drag overwhelming the lesser dragging force in high latitudes and reversing its motion, a velocity of 2 miles an hour eastward instead of westward resulted in the polar half, and the normal equatorial velocity of 4 miles was reduced to 2 miles per hour; so that in relation to the earth's surface the dragging force, normally represented by the foregoing figure of 20, became reduced to $2^2 + 2^2 = 8$: the friction in the equatorial half thus becoming the same as in the polar half, though in the opposite direction. These figures were given in a simple form to illustrate the manner in which the overwhelming of lesser by greater forces resulted in a decrease of resistance to the earth's rotation. The reversing of 2 miles per hour in the extra-tropical region resulted in the work done in that region being equivalent to 4 miles per hour, and only to 2 in the equatorial region—together a normal quantity of 6; but the frictional resistance on the earth's surface was reduced to less than half, as shown by the foregoing figures.

The idea of the main features of oceanic circulation being due to the winds was now not universally accepted, the newer view regarding it as erroneous; and, as a matter of fact, if the East Australian coast current came from the position indicated in Fig. 1, Plate 5, it would not be so

distinctly a warm current. Admiral Wilkes,[1] of the United States Navy, had first declared that stream to come from the east of New Caledonia, and Captain Maury[2] was of the same opinion. But Wilkes' own temperature chart showed the origin given in Mr. Halligan's figure to be a cold region of water flowing northwards from the east of New Zealand; and the 'Challenger' records have since shown that there existed a great mass of water flowing from the equator west of the longitude of New Caledonia decidedly warmer than the water east of that longitude. Admiral Lutke,[3] of the Russian Navy, showed that in the south-eastern trade-wind region of the Pacific Ocean there was no current flowing with the wind. A. G. Findlay[4] records that a counter-current in the North Pacific ran steadily right against the trade-wind for two weeks. Admiral Wilkes[5] further showed that in the region of St. Helena, in the heart of the south-east trade-winds of the South Atlantic, little or no current is ever experienced.

Finally, Major Rennell[6] had previously pointed out that experienced ship-captains knew that in the North Atlantic there was a broad stream which helped them when beating northwards against the north-east trade-wind.

[1] *United States Exploring Expedition*, by Charles Wilkes, U.S.N., 1845, p. 473.

[2] *Physical Geography of the Sea*, General Chart of Currents.

[3] *Voyage autour du Monde*, Frédéric Lutké, Partie Nautique, Saint-Pétersbourg, 1836, p. 186. 'En latitude de 26° nous reçumes un vent qui passa insensiblement à l'état de véritable vent alizé, et qui même quelquefois souffla fraîchement; mais tout cela ne produisit point de courant.'

[4] *North Pacific Ocean Directory*, by A. G. Findlay, 3rd ed., 1886, p. 1200. 'On the equator in longitude 175° E. a current of about 2 or 3 knots an hour ran to the eastward for 14 or 15 days, although the wind was then fresh from the eastward.' Findlay, on the same page, records that Admiral Wilkes traced the North Pacific counter-current from the long. of 170° E. to the long. of 138° W. Maury, in paragraph 401 of the work above quoted, says that the currents of the calm belt of the Pacific are very strong, and are generally found setting to the *west*.

[5] Page 469 of the work already quoted.

[6] *An Investigation of the Currents of the Atlantic Ocean*, by Major James Rennell, 1832, p. 115.

The records by Wilkes, Maury, and Lutke merely showed the impotence of the trade-winds in South Atlantic, South Pacific, and North Pacific to be the same as described by Rennell in the North Atlantic. The 'Roaring Forties,' alluded to by Mr. Halligan, created long sweeping seas in the South Atlantic, where the eastward current caused by the earth's rotation went with them; but when they met the Agulhas current coming from the east the character of the great waves changed. The current chafed against the wind, forming short, dangerous seas, but it held to its course unchecked; showing that the 'Roaring Forties' had as little to do with the creation of the eastward currents of the temperate zones as the trade-winds had with the westward currents of the equatorial regions. Mr. Halligan's Fig. 7 gave correctly the main features of the Australian coast currents, but it was certainly incorrect as regards the region from which the warm water on the east coast was derived.

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